

# Three Essays in Development Economics

by

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To my parents, 王青云 and 于清友.

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## ABSTRACT

This dissertation consists of three chapters. They study two topics in the context of a developing economy: how households make health-related decisions and how firms make use of political connections.

Chapter One aims to understand the role of social stigma in the HIV epidemic. Public health experts have seen the stigma as a leading barrier affecting the delivery of HIV-related health care. This chapter uses a field experiment in Mozambique to tackle this issue. To obtain local measures of the HIV stigma environment in the study sites, I conducted a baseline survey one year before the experiment. Experiment participants with excessive concerns, defined as overestimating the stigma in their communities, were randomly assigned an intervention to relieve stigma concerns. The intervention, which drew upon findings from the baseline survey, was designed to reveal the correct degree of stigma that a participant had overestimated. Analyses show that this intervention raised the HIV test uptake rate by 7.7 percentage points (or by 37 percent) from 20.7 percent under the control condition. To quantify the intervention effect, I introduced testing coupons of different values to estimate the demand curve for an HIV test. The concern-relieving intervention raised an individual's willingness-to-pay for an HIV test by \$1.30 or more than half of the daily cost-of-living in the study population.

Chapter Two evaluates a prominent effort to help families cope with HIV/AIDS: a U.S. government program in Mozambique, "Strengthening Communities and Children" (or Portuguese abbreviation, FCC), that implements home visits alongside a set of complementary interventions. This chapter focuses on the primary outcome of HIV testing, and two key mechanisms: improvements in HIV-related knowledge, and reductions in HIV-related stigmatizing attitudes. Causal identification exploits multiple levels of random assignment, most prominently of entire communities to FCC program receipt or a control group. We find that the FCC program has positive but small effects on HIV testing. Treatment effects are only one-fifth the magni-

tude of, and statistically significantly smaller than, the average of expert predictions elicited in advance. Likely mechanisms behind these modest effects are that the program worsened some aspects of households' HIV-related knowledge, and also worsened HIV-related stigmatizing attitudes. Additional treatments randomly assigned at the household level during our follow-up survey further highlight the role of these mechanisms: treatments improving knowledge and alleviating stigma concerns raise the impact of the FCC program on HIV testing.

Chapter Three focuses on a new context and studies the value of political connections in China. Inviting a government official to sit on the board is a commonly used strategy for firms seeking to become politically connected. This chapter estimates the value of this type of political connection with a nationwide, targeted policy shock in China. In October 2013, the central government announced a new policy that restricted government officials from working in firms. Firms with government-official outside directors were affected. I find that government-official outside directors do add to firm value: The stock return of affected firms is, on average, eight percentage points lower than that of the control firms in the 12 months following the policy change. The variation in treatment effects across firms suggests that firms rely on this type of political connection to different degrees. Several potential working mechanisms are explored.

## CHAPTER I

# Social Stigma as a Barrier to HIV Testing: Evidence from a Randomized Experiment in Mozambique

### 1.1 Introduction

In 2013, the United Nations called for ninety percent of all people living with HIV to know their status by 2020 in its 90-90-90 goal.<sup>1</sup> A month shy of 2020, however, this goal will not be met. In 2018, the last year for which data was collected, only 79 percent of the global infected population knew their status. Insufficient status-awareness matters because it imposes extraordinary challenges on public health authorities to prevent transmission and expand medical treatment.

Crucial to overcoming this challenge is to raise the HIV testing rate, and especially in Sub-Saharan Africa, which remains the world's most HIV-affected region. Of the 37.9 million people living with HIV, 25.6 million are from Sub-Saharan Africa. While global donors, through a decade-plus of collaboration with local partners, have made HIV testing freely accessible in almost all of Sub-Saharan Africa, a low test-uptake rate has substantially undermined this supply-side effort.<sup>2</sup>

Medical practitioners and community leaders often blame the stigma attached to HIV for the low testing rate. Anecdotal evidence suggests that people have avoided HIV testing for fear of being seen and stigmatized by their neighbors. Although public health scholars have documented correlations between high degrees of stigma

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<sup>1</sup>The three specific goals are: "By 2020, 90% of all people living with HIV will know their HIV status; 90% of all people with diagnosed HIV infection will receive sustained antiretroviral therapy; 90% of all people receiving antiretroviral therapy will have viral suppression."

<sup>2</sup>Data source: UNAIDS AIDS information program: <http://aidsinfo.unaids.org>

and low testing rates under various circumstances (Sambisa, Curtis and Mishra, 2010; Berendes and Rimal, 2011; Maughan-Brown and Nyblade, 2014; Kelly, Weiser and Tsai, 2016) , there is a dearth of well-identified evidence on the causal effect of stigma on HIV testing. Nor do we know how large an impact the stigma imposes on an individual’s testing behavior. The main challenge to causal identification is that stigma, as a parameter of society, is difficult to experimentally alter without altering confounding factors at the same time.

My paper overcomes this challenge by employing an intervention that tackles concerns for stigma at the individual level.<sup>3</sup> I use a randomized control trial (RCT) in Mozambique for two purposes: to identify the role stigma concerns play in hindering HIV testing and to quantify the stigma barrier. To obtain local measures of the social stigma attached to HIV, we conducted a baseline survey in the study communities one year before the RCT. Participants of the RCT estimated the degree of stigma in their community before entering a randomization process. Those with excessive stigma concerns, i.e., overestimated stigma in their community, were randomly assigned to receive an intervention to alleviate concerns. The concern-relieving intervention, which was individually tailored, revealed the true degree of stigma that a participant had overestimated. We then tracked test-seeking behavior.

I find that the concern-relieving intervention raised the participants’ test uptake rate by 7.7 percentage points, or by 37%, from 20.7 percent under the control condition. This experiment provides clear evidence that the stigma concerns for stigma are a barrier that has caused people to avoid taking HIV tests.

To quantify the stigma barrier, I introduce different levels of monetary incentives for HIV testing. The testing service in Mozambique is free and anonymous. To track individuals’ test-seeking behavior, I offered all study participants coupons (a conditional cash transfer) to take tests. The Control Group and the Concern-Relieving

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<sup>3</sup>I follow the conceptual work of Goffman (1963) and define the stigma attached to HIV as the phenomenon that people living with HIV are socially avoided. Accordingly, the concerns for the stigma are individuals’ concerns for being avoided in social life because of their association with HIV. The stigma measures and interventions used in this study strictly followed this definition.

Public health scholars have discussed the concept of the stigma attached to HIV more broadly (Parker and Aggleton, 2003; Stangl, Brady and Fritz, 2012; Stangl et al., 2013). According to previous conceptual work, the broad concept of stigma has manifestations beyond social avoidance, such as internal stigma (feel ashamed of oneself) and enacted discrimination (be assaulted or treated unfairly by others). My study adopted a narrower working definition of stigma to allow for rigorous quantitative analyses. Social avoidance is the core manifestation of all stigmas and can be measured in my study setting. The rise of social avoidance is not the focus of this study; it could stem from the moral judgment on the infected person or people’s excessive concerns for infection. See Stangl, Brady and Fritz (2012) and Stangl et al. (2013) for reviews.



Intervention Group received coupons of 50 Meticaïs (2.25 dollars by PPP), which was equivalent to the daily cost-of-living. An additional study group, the High-Incentive Group, was introduced in parallel, where participants received no intervention but coupons of 100 Meticaïs. The Control Group and the High-Incentive Group locally pin down the demand curve for an HIV test. On the demand diagram, relieving stigma concerns raised individuals’ willingness to pay (WTP) for an HIV test by 29 Meticaïs (1.30 dollars by PPP).

My paper contributes to the literature on understanding HIV testing behavior in developing countries. While many studies in this literature have focused on exploring practical interventions to promote HIV testing, they have often paid less attention to investigating the mechanisms or identifying a specific barrier inhibiting HIV testing. For example, researchers have found that financial incentives and home-base testing delivery could raise the testing rate (Swann, 2018; Moshoeu et al., 2019), and have argued that alleviating stigma was a working channel. Nevertheless, as both interventions addressed multiple potential barriers at the same time,<sup>4</sup> we still do not know which barriers prevent individuals from seeking a test or the best ways to overcome these barriers. A recent effort to identify the barrier of stigma concerns is Derksen and van Oosterhout (2019). They found that disseminating educational messages in a community raised the HIV testing rate and argued that reducing the residents’ stigma concerns was the mechanism. The stigma’s role in their study, however, was not directly supported by experimental evidence.<sup>5</sup> Confounding mechanisms could still drive the effect, such as people inferring higher medication effectiveness or acting altruistically. The lack of knowledge on specific barriers obstructs us from learning the underlining motivations behind human behavior and prevents us from designing cost-effective interventions to fight HIV.

In contrast to the existing studies, my experimental intervention directly and solely manipulates an individual’s stigma concerns. Any observed effect on the testing rate

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<sup>4</sup>Providing financial incentives and delivering home-based testing services may both address several barriers at the same time: monetary cost, by compensating or avoiding transportation fees and loss of time; procrastination, by offering instant incentives for testing or reducing cost; stigma concerns, by concealing the intrinsic motivation to learn one’s status or avoid being seen by others (Thornton, 2008; Feyissa, Lockwood and Munn, 2015; Swann, 2018; Moshoeu et al., 2019).

<sup>5</sup>Derksen and van Oosterhout (2019) argued with suggestive evidence that their informational intervention—health education meetings disseminating the effectiveness of the HIV treatment in preventing transmission—made people think that their community became more aware that HIV positive persons on medication have a low chance of transmitting HIV. Hence, people in their intervention group had fewer concerns for “statistical discrimination” by potential sex partners, and, as a result, sought more tests.

can be traced back to relieved stigma concerns. The clear-cut design allows me to establish the causal effect of the stigma concerns on testing and to quantify its impact.

My study also contributes to the literature on the role of stigma in socio-economic life. Stigmas widely exist in human society and increasingly attract economists' attention. Some earlier work theoretically analyzed the rise and implications of stigma related to social welfare receipt and divorce (Moffitt, 1983; Besley and Coate, 1992; Ishida, 2003). A strand of empirical literature studied how stigma concerns affect individuals' decisions to claim welfare and reached mixed conclusions (Bhargava and Manoli, 2015; Friedrichsen, König and Schmacker, 2018).

The stigma is especially widespread in the realm of public health. Many health conditions are stigmatized (Puhl and Heuer, 2009; Bharadwaj, Pai and Suziedelyte, 2017), HIV infection being a common and policy-relevant example. Hoffmann, Fooks and Messer (2014) documented evidence of the stigma attached to HIV: The general population tended to avoid objects touched by people living with HIV. In my study, I take a step further to show that stigma concerns can cause behavioral changes in the vulnerable population and lead to real health and economic consequences.

In addition, my work relates to the literature on how misperceived social parameters affect human behavior (Jensen, 2010; Cruces, Perez-Truglia and Tetaz, 2013; Armona, Fuster and Zafar, 2018). The intervention tool I use is built on a type of "norm-based interventions," which alter people's perceptions of certain social norms by revealing summary statistics of behavior in a reference group (Benabou and Tirole, 2011). Researchers have used "norm-based interventions" to study individuals' reactions to learning social norms in energy consumption (Schultz et al., 2007), female labor force participation (Bursztyn, González and Yanagizawa-Drott, 2018), and attitudes toward healthy sexual relationships (Banerjee, Ferrara and Orozco-Olvera, 2019). The study setting of Banerjee, Ferrara and Orozco-Olvera (2019) was the closest to mine. In their experiment, young participants in Nigeria first viewed an entertainment-education TV series promoting healthy sexual relationships and then reported their attitudes towards the TV contents. The treatment group was informed of their peer's average post-view attitudes before they reported their own. The authors did not find that the intervention of revealing peer's attitudes affected participants' attitudes.

Unlike previous studies, my intervention identifies new parameters beyond the effect of social norms. I use social opinion statistics as a tool to mitigate a psychological barrier, i.e., the stigma concerns. The outcome of interest, taking up an HIV test,

is a behavior differing from the one in which I reveal summary statistics. My paper shows how social opinions collected from hypothetical questions can affect the decision on behavior of high stakes. Moreover, I combine the norm-based intervention with varying financial incentives to quantify the intervention effect.

## 1.2 Background

### 1.2.1 The HIV Epidemic and Testing Services in Mozambique

HIV prevalence in Mozambique reached 12.6% among adults in 2018, making it one of the countries most affected by the epidemic. Mozambique has fallen behind the United Nations' 90-90-90 goal in each step of the HIV treatment cascade. Only 72% of all people living with HIV in Mozambique know their status. This low awareness has become a major obstacle for HIV treatment and prevention. In 2018, there were 150,000 new HIV infections and 54,000 AIDS-related death in Mozambique, accounting for one-twelfth and one-fourteenth, respectively, of the global totals.<sup>6</sup>

Mozambique built up its nationwide standardized HIV-testing (formally known as Health Counseling and Testing) service system following WHO guidelines. Beginning in 2008, the Mozambican government integrated HIV-related services into other clinical services in sanitary units (US) in communities. HIV-related services and materials are free of charge in all US's.

In 2017, Mozambique conducted 7,866,465 HIV tests. (The ratio of the number of tests to population is 0.273.) The majority, or 80.2%, of the tests were conducted through the provider-initiated counseling and testing (Portuguese abbreviation ATIP) approach, where doctors referred patients with symptoms of infections to take tests for HIV. The ATIP approach is typically only able to catch HIV infections late in the progress when the virus may already have transmitted to others, and the patient has missed the best window to initiate medical treatment. Only 12.7% of HIV tests were initiated by general residents who voluntarily sought to learn their status in a sanitary unit (formally known as the user-initiated counseling and testing approach, Portuguese abbreviation ATIU).<sup>7</sup> In a high HIV-prevalence region like Mozambique, encouraging the general population to learn its status before any sign is shown is

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<sup>6</sup>Data source: UNAIDS (2019).

<sup>7</sup>Data source: "Annual report on activities related to HIV/AIDS 2017" National Healthcare Service, Mozambican Ministry of Health (Portuguese: *Relatório Anual 2017 Relatório Anual das Atividades Relacionadas ao HIV/SIDA*).

essential for preventing transmission and improving treatment efficacy. In this study, we collaborated with the local sanitary units, where we refer eligible participants to take HIV tests through the ATIU approach and track their testing behavior.

### 1.2.2 Study Population

My research experiment is embedded in a broader evaluation study of the anti-poverty program *Força à Comunidade e Crianças* (FCC, “Strengthening Communities and Children”) in Mozambique.<sup>8</sup> Our research team conducted a household survey, hereafter the baseline survey, between May 2017 and March 2018 in 76 communities across three provinces in central Mozambique. The baseline survey covered a population-representative sample in each study community and collected rich information about household members’ health, education, knowledge about HIV, and social opinions. The experiment analyzed in this paper was conducted on an economically disadvantaged subset of the baseline survey sample. 71.6% of the baseline households are categorized as “vulnerable” according to a list of pre-specified criteria, and they constituted the pool of potential participants for this experiment.<sup>9</sup>

### 1.2.3 Measures of the Stigma Environment

We constructed three measures of stigma environment within each community by summarizing the baseline survey responses to each of the following questions.

- Q1.** Would you buy fresh vegetables from a shopkeeper if you knew that this person had HIV? (Yes/No)
- Q2.** If a member of your family became sick with AIDS, would you be willing to care for them in your own household? (Yes/No)
- Q3.** In your opinion, if a teacher has HIV but is not sick, should they be allowed to continue teaching at school? (Yes/No)

The questions assess an individuals’ tendency to avoid people living with HIV (stigmatize HIV); an affirmative answer indicates a supportive attitude, while a neg-

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<sup>8</sup>See Yang et al. (2019) for an extensive discussion of the FCC program.

<sup>9</sup>We assessed a household’s vulnerability in 11 dimensions that covered income, food security, adult-to-child ratio, and health conditions. Please see Appendix C for details. Since the baseline survey sample is population-representative, the participant pool can be considered the bottom 71.6% of Mozambique’s population in the economic well-being distribution. As a comparison, the poverty headcount ratios at \$1.90 (2011 dollar) a day and \$3.20 (2011 dollar) a day are 62.4% and 81.5% of the country’s population, respectively. Hence, people in my participant pool roughly lived on \$1.90 to \$3.20 a day. Data source: World Bank Data.

ative answer indicates stigmatization. A higher fraction of affirmative responses from a community indicates a local environment with less stigma.

The major takeaway from the baseline environment assessment is that the fraction of respondents giving affirmative answers was high across all communities, indicating low social stigma attached to HIV. In an average community, the fractions of respondents giving an affirmative answer to the three questions were 80.1%, 93.2%, and 89.2%, respectively. The variation across communities is moderate, and except for Q1 in three communities, the supportive fractions are always higher than 60%.<sup>10</sup> The three community-level stigma measures are used in the experimental intervention discussed later to mitigate participants' concerns for stigma.

The worldwide panel AIDS Indicator Survey (AIS) has used the same three questions to monitor HIV-related stigmas. The low stigma finding from the baselines survey is consistent with the findings from the AIS panel in Mozambique. The four rounds of AIS between 2003 and 2015 show a trend of rapidly lowering stigma associated with HIV in Mozambique. Figure 1.1 presents the four rounds of AIS and our baseline sample together.

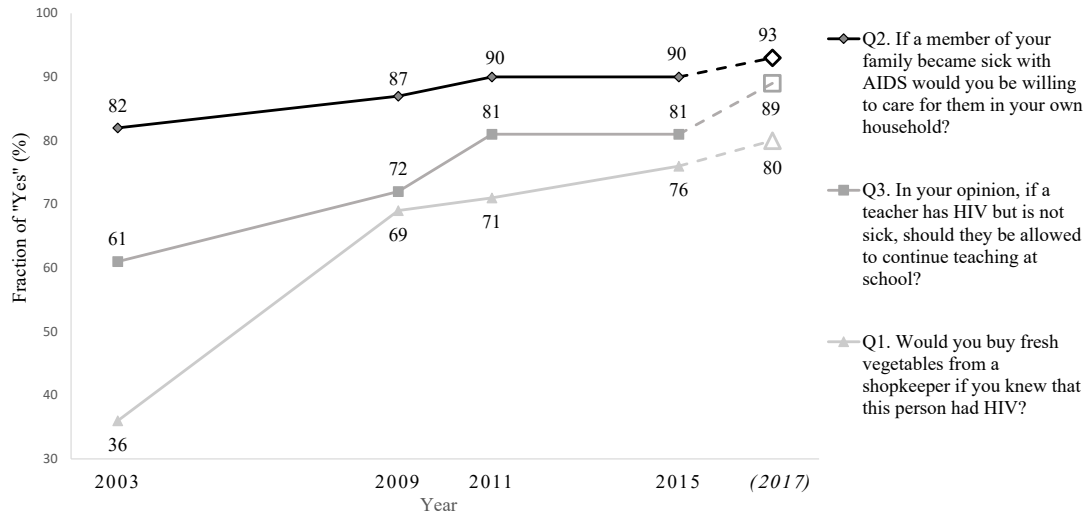


Figure 1.1: Stigma Environment Measures in Mozambique over Time

**Notes:** Data points for 2003, 2009, 2011, and 2015 are calculated from the nationally representative sample of Mozambique AIDS Indicator Survey by USAID. Data points for 2017 are calculated by taking a simple average from the baseline survey in this study.

<sup>10</sup>See Appendix D for the three measures in each study community.

## 1.3 Experimental Design

### 1.3.1 Recruitment Survey and Stigma Concern Assessment

The experiment was rolled out between May and October in 2019. To recruit participants, enumerators visited a list of prespecified “vulnerable” households from the baseline sample. Adults available at the time of home visits first answered a survey and then, depending on their survey responses, were assigned to a study group at random.

The purpose of the survey upon recruitment is threefold. First, it collected testing histories and screened eligible individuals to receive HIV test coupons. Survey respondents who were already known to be HIV positive or had been tested within the last three months were not offered coupons, and, thus, excluded from the RCT.<sup>11</sup> 62% of the surveyed people were eligible for our testing coupons; 14% were not eligible because they self-reported to be HIV positive; 24% were not eligible because they had been tested negative within 3 months before the survey. Second, in the survey, we assessed participants’ concerns for stigma. Participants who overestimated social stigma in their community were subsequently randomized to the Concern-Relieving Intervention Group or the Control Group. Lastly, the recruitment survey collected a rich set of pre-intervention characteristics of the participants.

During the recruitment survey, we assessed each respondent’s concerns for the stigma attached to HIV before randomly assigning them to different experiment conditions. The baseline survey delivered the encouraging news of a low stigma environment; however, people may lack accurate knowledge about the environment in which they live. In fact, it is not the true stigma environment, but people’s *beliefs* about the stigma environment that concerns them and may affect their test-seeking decision.

In the recruitment survey, we ask participants to report their beliefs about the three stigma measures of their community:

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<sup>11</sup>The government-recommended frequency of testing for the general population is once per six months. See “National Guideline for the Implementation of the Counseling and Testing in Health” issued by the Mozambican Ministry of Health in 2017. (Portuguese: *Directriz Nacional Para a Implementação do Aconselhamento e Testagem em Saúde*.)

- EQ1.** If I ask the question, “Would you buy fresh vegetables from a shopkeeper if you knew that this person had HIV?” to 10 people in your neighborhood, how many of them would you expect, to say “Yes”?
- EQ2.** If I ask the question, “If a member of your family became sick with AIDS, would you be willing to care for them in your own household?” to 10 people in your neighborhood, how many of them would you expect, to say “Yes”?
- EQ3.** If I ask the question, “In your opinion, if a teacher has HIV but is not sick, should they be allowed to continue teaching at school?” to 10 people in your neighborhood, how many of them would you expect, to say “Yes”?

If a *belief* is lower than the corresponding *truth* in her community, then this participant has overestimated stigma in this measure. For example, if in the baseline survey, 90% of the respondents in a community said “yes” when asked if they would buy fresh vegetables from a shopkeeper whom they knew to have HIV, but a participant believed that only 70% of people in her community would have said “yes” to the question, then this participant had *overestimated* stigma in her community.

Table 1.1 summarizes participants’ beliefs in the recruitment survey and compares them with statistics from the baseline. The coupon-eligible sample, on average, believed that 70.2%, 77.5%, and 81.4% of their neighbors would give affirmative answers to the three stigma-measuring questions. These numbers are significantly lower than the fractions collected from the baseline. Figure 1.2 depicts the distribution of bias of participants’ beliefs about the stigma measures. The bias is defined as a participant’s belief minus the true fraction of people in her community giving affirmative answers in the baseline survey. A negative bias indicates overestimating stigma.

A participant is defined as “concerned” for social stigma if she overestimated at least one of the stigma measures in her community. 62.7% of the coupon-eligible sample fell into this category. Individuals in this category constituted the primary analysis sample and were randomly selected to receive the concern-relieving intervention.

At this point, we can glance at the correlation between the stigma concerns and past test uptake behavior in our study population. For everyone in the survey sample, we take an average of her guesses of the three stigma measures to obtain an individual “stigma perception” measure. Figure 1.3 divides survey respondents to quintiles by their beliefs about stigma measure and depicts the self-reported testing rate (the fraction of people ever tested for HIV) of each quintile. Those with fewer stigma concerns (i.e., those who guessed a high fraction of people giving affirmative answers to

Table 1.1: Beliefs and Truths of the Stigma Environment

Stigma Measure Question	Obs.	Mean belief	Truth from the baseline survey	p-value of ttest: belief = truth	Share overestimated stigma in this question
Q1	1,392	70.2%	79.4%	<0.001	46.0%
Q2	1,397	77.5%	92.8%	<0.001	51.3%
Q3	1,402	81.4%	88.8%	<0.001	40.8%
Overestimated stigma measured in at least one of the three questions					62.7%

**Notes:** This table reports the beliefs of the coupon-eligible sample (sample size = 1,588). The fraction of respondents answered "yes", Column (3), is calculated by reweighing the baseline sample to match the geographic distribution of the experiment participants, Column (2). When calculating the fraction of coupon-eligible participants that overestimated stigma measured in at least one of the three questions, missing beliefs are treated as "not overestimate". If we drop the missing beliefs, the fraction is 70.2%.

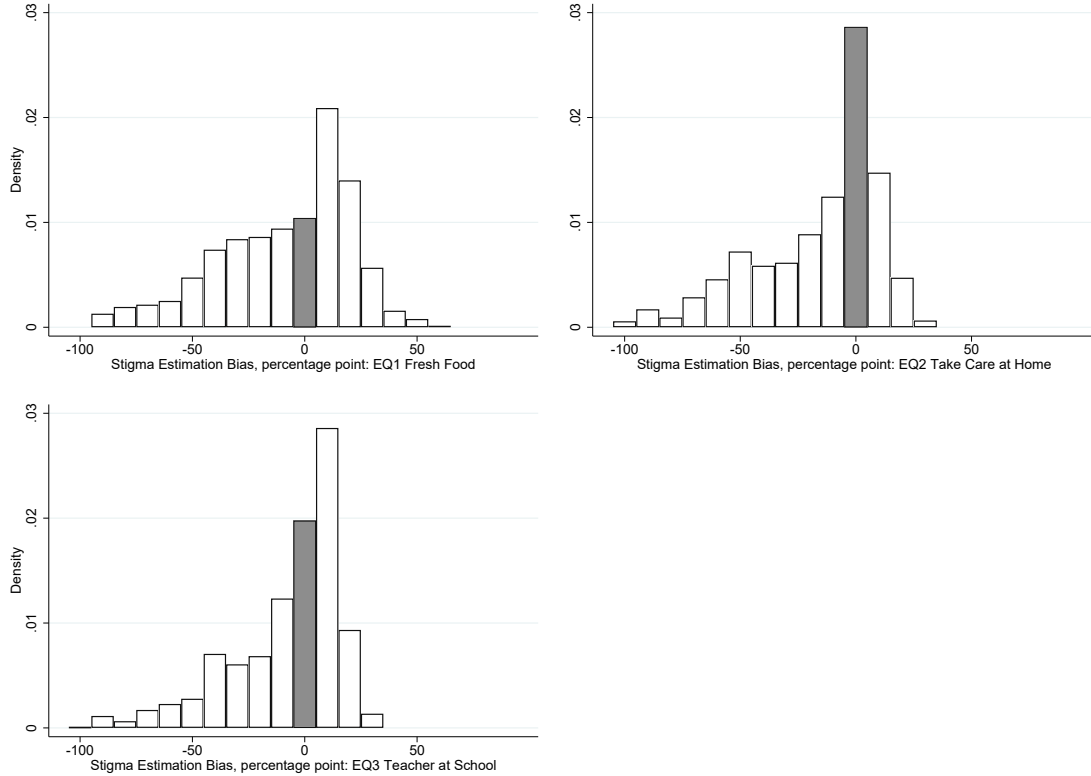


Figure 1.2: Distribution of Participants' Bias in the Belief about Stigma

**Notes:** A bias is defined as an individual's belief about a stigma environment measure (an individual's answer to question EQ1, EQ2, or EQ3, transformed to percent, i.g. 6 out of 10 is transformed to 60 percent) minus the true stigma measure obtained from the baseline survey in her communities (summary of question Q1, Q2, or Q3). A negative bias indicates overestimating stigma. The histograms are based on all participants that are eligible for coupons.



Q1 to Q3) are in quintile 1 while the most concerned people are in quintile 5. Quintile 1 has a significantly higher testing rate compared to other quintiles. The pattern in Figure 1.3 echoes previous findings that greater concerns for stigma are associated with a lower HIV testing rate, but causality remains unknown. This association could come from a hidden third factor that caused high stigma concerns and fewer test uptakes at the same time or derive from the inverse causal channel that learning one's HIV status leads to lower stigma concerns. To rule out these hypotheses, we introduced the concern-relieving intervention to experimentally mitigate the stigma concerns of a random subset of participants.

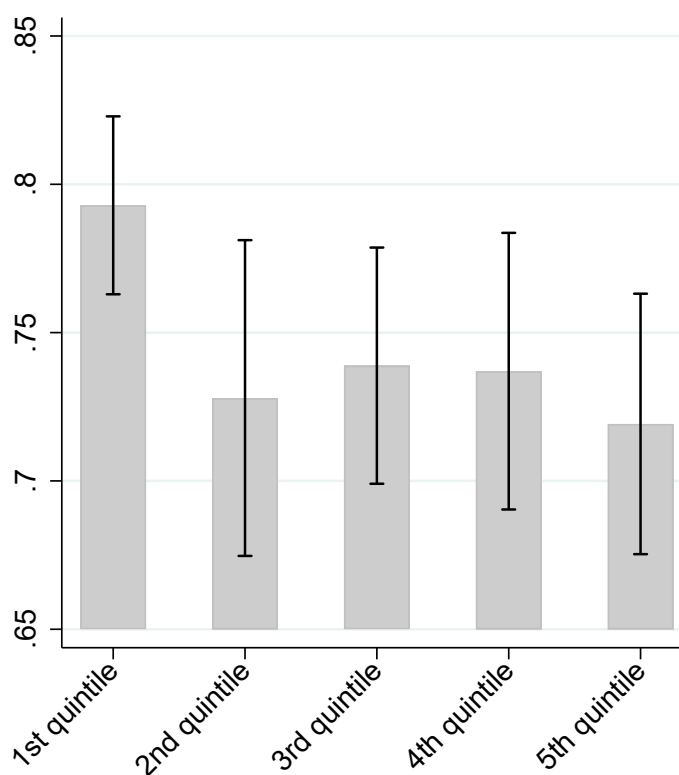


Figure 1.3: HIV Testing Rate by Belief about Stigma

**Notes:** The *y-axis* is the fraction of recruitment survey respondents self-reported to had ever tested for HIV. The *x-axis* is the quintile groups of the belief about local stigma measures. An individual's belief is the average of her answers to question EQ1, EQ2, and EQ3, a higher average indicates that this person perceived less stigma. Individuals are ordered by their beliefs, with those who perceived the least stigma on the left and those perceived the most stigma on the right. The first quintile group includes the left-most 20 percent, and so on.

### 1.3.2 The Concern-Relieving Intervention

This intervention shares the measures of stigma environment from the baseline survey with the participants. To protect human subjects, the information sharing is asymmetric – a true measure was revealed to a participant only when she “overestimated” stigma in that measure, but not when she correctly estimated or underestimated it.

One-third of the “concerned” participants received the concern-relieving intervention by random selection. The intervention was administered after the recruitment survey. The enumerator revealed measure(s) of the stigma environment that the participant had overestimated and explained the implications. As an example, a piece of the enumerator’s scripts for the interventions goes as follows:

*I’d like to share with you some information we collected from your neighborhood. Recall that a few minutes ago, I asked you to guess, out of 10 people, how many of them would have answered “yes” to the following question:*

*“Would you buy fresh vegetables from a shopkeeper if you knew that this person had HIV?”.*

*Your guess was [6 out of 10] people would answer “yes.”*

*In fact, we did ask a large number of people this question last year in your neighborhood. The fact is [more than 9 out of 10 people (or 91.5%)] answered “yes.” People in your community are more acceptive of people infected with HIV than you thought they would be.*

The intervention shared one to three pieces of such information depending on the number of overestimates a participant made in the concern-assessing process.

### 1.3.3 Coupon Distribution

The primary outcome of interest is the HIV test uptake. In Mozambique, HIV tests are voluntary and anonymous. We adopted coupons to track participants’ test-seeking behavior. HIV testing is free of charge at the local clinics. Hence, a coupon should be considered a conditional cash transfer. After a participant completed the survey and the concern-relieving intervention (when applicable), the enumerator distributed a testing coupon to each participant who was not already known to be HIV positive and had not been tested within the last three months.

We varied the value of the coupons to pin down the effect of monetary incentives on testing. A regular-value coupon was worth 50 Meticaïs (2.25 dollars by PPP), and a high-value coupon was worth 100 Meticaïs. As discussed in the previous section, the study population roughly matches the population in absolute poverty in Mozambique who lived on \$1.90 to \$3.20 (2011 dollars) a day. A low-value coupon had the value of an average participant’s daily cost-of-living.

In addition to one coupon for each adult participant, we also distributed coupons of the same value for each of the participant’s eligible children. A child followed the same eligibility criteria for a coupon as an adult. The enumerator informed the participant that a coupon should only be used by the designated person (the adult coupon for the participant himself or herself and the child coupons for any eligible children). For the participant’s convenience, coupons for an adult male, adult female, and a child were of different designs.

At the time of distributing coupons, we also informed the participants of the typical time costs of testing in the local sanitary units, and the method of payment for coupon redemption. To avoid any pressure or concerns for confidentiality loss, we confirmed that at the time of coupon redemption, the study team would not collect any individually identifiable information or ask for their test results.

#### **1.3.4 Randomization Structure and Balance Test**

We jointly randomized the concern-relieving intervention and the value of coupons. Figure 1.4 summarized the group structure.<sup>12</sup> Participants of the Concern-Relieving Intervention Group received regular-value coupons. The rest of the “concerned” participants were randomized to two groups: the Control Group, who received regular-value coupons, and the High-Incentive Group, where high-value coupons were offered. The non-concerned participants were also randomized to receive regular-value or high-value coupons, but none received any information about stigma. The randomization was conducted at the household level. If more than one adult from a household were “concerned,” they received (or did not receive) the concern-relieving intervention at the same time. (When multiple adults in the same household were to receive the concern-relieving information, the information each person received was still individually tailored and delivered in private.) All members from the same household,

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<sup>12</sup>Please see for details in the recruitment and randomization procedures.

including children, received coupons of the same value. Table 1.2 reports summary statistics of the coupon-eligible sample and conducts balance checks.<sup>13</sup>

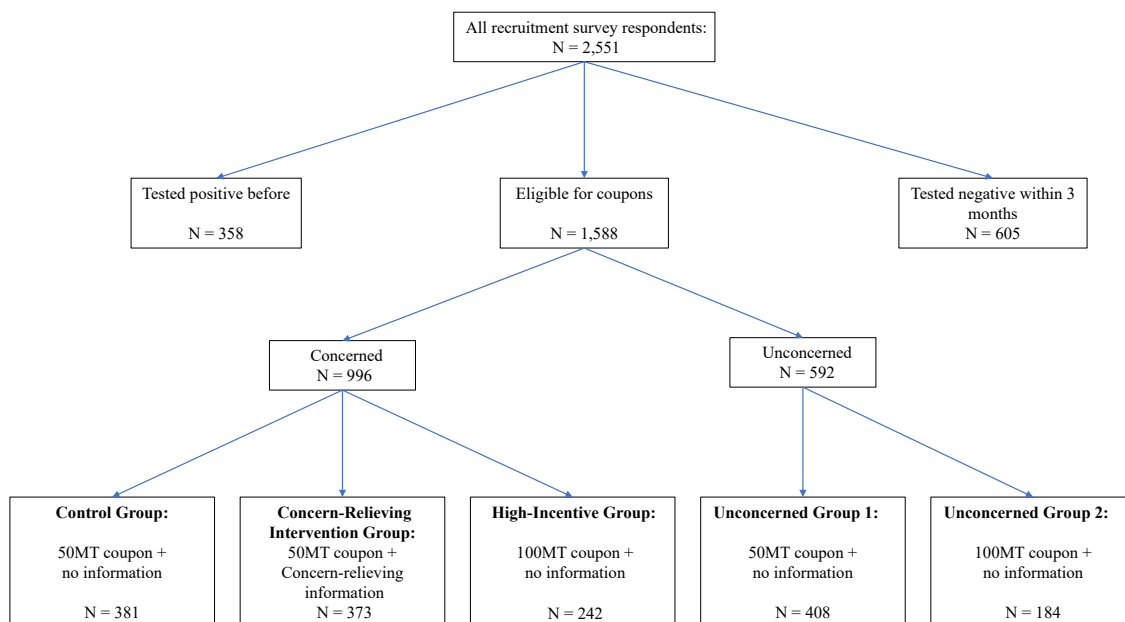


Figure 1.4: Experimental Design and Sample Structure

### 1.3.5 HIV Test and Coupon Redemption

All coupons were valid for 14 days. A coupon was redeemable at the designated sanitary units when someone presented proof of HIV-testing with the coupon to the research staff on site. The payment was made in digital cash through MPesa. There was a unique barcode on each coupon that allowed us to link the use of the coupon to one's survey responses.

To redeem a coupon, a participant should take an HIV test at a local sanitary unit. To ensure the convenience of testing, we involved all the commonly used sanitary units of the participating communities. They included but were not limited to all the geographically closest ones. When distributing the coupons, we encouraged participants to get tested in the closest sanitary units and promised staff presence in those units within the 14-day window. Participants, however, were able to redeem coupons at any sanitary units when our research staff was on site.

The HIV test in a sanitary unit is based on a 3-stage process.

<sup>13</sup>Please see Appendix A for summary statistics of other samples.

Table 1.2: Balance Table

Variables	Obs.	Control Group Mean (s.d.)	Diff: Concern- Relieving Intervention minus Control (p-value)	Diff: High-Incentive minus Control (p-value)
Indicator: female	996	0.685 (0.465)	0.002 (0.947)	0.007 (0.878)
Age	990	36.458 (16.088)	0.584 (0.622)	-2.878 (0.061)
Education in years	991	6.356 (4.005)	-0.418 (0.153)	-0.017 (0.964)
Indicator: is the primary guard of some child(ren)	996	0.696 (0.461)	0.027 (0.437)	0.032 (0.485)
Indicator: respondent provided a private phone number	996	0.541 (0.499)	-0.028 (0.457)	0.075 (0.108)
# of sex partners in the last 12 months: none	943	0.199 (0.400)	0.002 (0.954)	0.012 (0.748)
# of sex partners in the last 12 months: only one	943	0.667 (0.472)	0.017 (0.659)	0.008 (0.853)
# of sex partners in the last 12 months: more than one	943	0.134 (0.341)	-0.018 (0.488)	-0.020 (0.527)
HIV-test history: never tested	985	0.388 (0.488)	0.047 (0.220)	0.017 (0.707)
HIV-test history: tested more than one year ago	985	0.264 (0.441)	-0.011 (0.741)	0.006 (0.897)
HIV-test history: tested within one year	985	0.348 (0.477)	-0.036 (0.335)	-0.023 (0.615)
# of correct answers out of 15 HIV questions	996	11.919 (2.998)	-0.131 (0.581)	-0.449* (0.094)
Subject risk of HIV+: the higher the riskier	965	1.761 (0.920)	0.056 (0.389)	-0.034 (0.652)
Distance in km between the household and a clinic	973	2.128 (2.941)	-0.010 (0.791)	0.086* (0.054)
Indicator: household go without food in the past 12 months	996	0.564 (0.496)	-0.018 (0.629)	0.047 (0.266)
Indicator: household has HIV+ member	920	0.072 (0.258)	0.016 (0.484)	0.056** (0.038)
1st principal component of the ownership of 14 assets†	996	0.884 (2.085)	0.003 (0.986)	0.201 (0.262)

**Notes:** The p-values are from t-tests of equality. The t-tests are controlled for community fixed-effects and enumerator fixed-effects.

† The 14 assets are car, motorbike, bike, radio, TV, sewing machine, refrigerator, freezer, iron, bed, table, mobile phone, clock, and solar panel. I use the mean and standard deviation of each ownership indicator in the baseline sample to standardized the indicators. Loadings of each indicator to construct the first principal component are also obtained from the baseline sample.

1. *Pre-test counseling*

The health care provider will address HIV prevention strategies, assess the test taker’s risk behavior, and introduce possible services available regardless of the test result.

2. *Testing and counseling during testing*

The health care provider will perform a Rapid Test, explain how to interpret the different potential test results, and provide psychosocial support to face the test result.

3. *Post-test counseling*

The health care provider will review the test result with the test taker and encourage the testing of partners. Depending on the test results, the test provider will refer the test taker to follow-up health counseling and services.

All three stages were conducted one-on-one in the clinics involved in this study. The standard 3-stage process took around 30 minutes per person. At the end of the process, the doctor would sign a proof-of-testing slip for the test taker. The research staff on-site would pay the coupon value when a coupon was presented with proof of testing.

When redeeming the coupon, the research staff did not try to identify the coupon holder or link the coupon to any information collected from the survey. After the coupon value was paid, the redemption staff would scan the coupon barcode, take notes of the coupon holder’s gender and age range (below or above 18 years old), and ask where the coupon was from. The coupon barcode and coupon holder’s information were later linked to the recruitment survey responses.

## 1.4 Experimental Results

### 1.4.1 Main Result - Test Uptake

Comparing the test uptake rate of the Control Group and the Concern-Relieving Intervention Group identifies the intervention effect, which in turn reveals the role of stigma concerns on testing.

Figure 1.5 compares the raw test uptake rate of the two groups. Test uptake is defined as a coupon distributed to a participant being used within 14 days by an adult of the same gender and self-reported as the original coupon recipient. 20.7 percent of the participants under the control condition take up a test. The test uptake rate

increases to 27.1 percent with the concern-relieving intervention. (The p-value of a t-test of equality is 0.0441.)

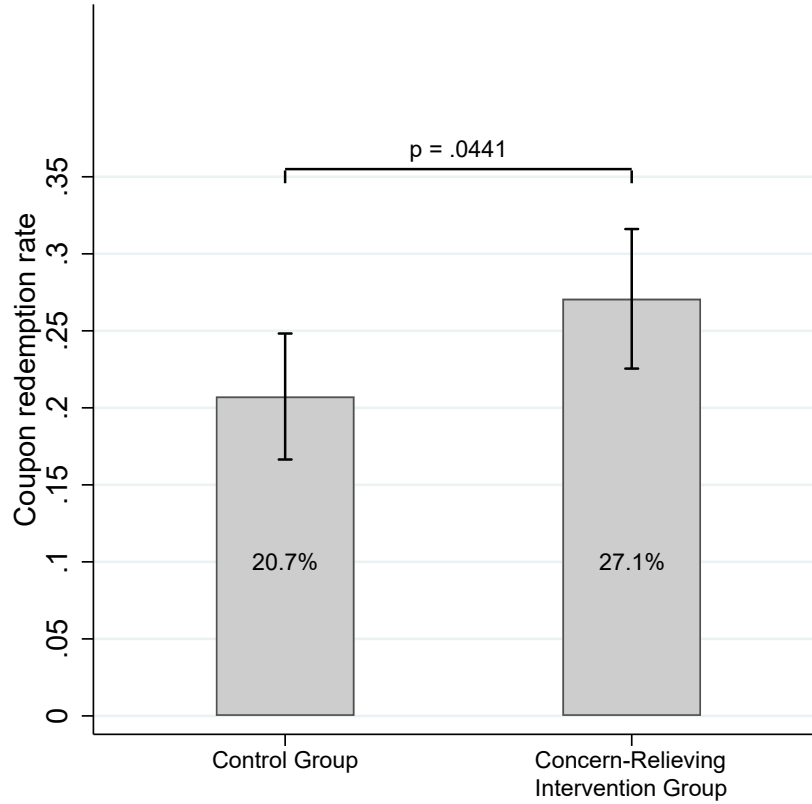


Figure 1.5: Test Uptake Mean Comparison: Control and Intervention

**Notes:** The *y-axis* is the HIV test uptake rate measured by the fraction of participants redeemed testing coupons. Both the Concern-Relieving Intervention Group and the Control Group received coupons of the value of 50 Meticaïs.

Table 1.3 presents the finding in the regression format. Column (1) replicates Figure 1.5. Column (2) shows the result from regressing Equation (1.1) that controls for pre-intervention characteristics. The estimated intervention effect is stable between the two specifications.

$$Y_i = \alpha + \beta G_i^{relieve} + \mathbb{X}_i + \epsilon_i. \quad (1.1)$$

$Y_i$  is an indicator for individual  $i$  taking up an HIV test.<sup>14</sup>  $G_i^{relieve}$  is the indicator of receiving the concern-relieving intervention (as opposed to being assigned

<sup>14</sup>A participant is coded as tested if and only if the assigned coupon was used within 14 days after distribution and was used by an adult of the same gender of the original coupon recipient who self-reported as an original coupon recipient. See Appendix B for robustness checks of definition variations.

to the Control Group).  $\mathbb{X}_i$  is the vector of individual characteristics.  $\epsilon_i$  is the error term clustered at the household level. Equation (1.1) applies to the union of the Control Group and Concern-Relieving Intervention Group sample.<sup>15</sup> The estimated intervention effect 7.7 percentage points.

Table 1.3: Main Result: The Effect of the Concern-Relieving Intervention

Group Indicator	(1)	(2)
	Test Uptake	
Concern-Relieving Intervention	0.0634** (0.0315)	0.0771** (0.0326)
Control Group Mean	0.207	0.207
Observations	754	754
R-squared	0.006	0.292
Constant	yes	yes
Controls	no	yes

**Notes:** Standard errors in parentheses. Standard errors are clustered at the household level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

The control variables are: Indicator: a participant is female (yes, no); Indicator: a participant is the primary guardian of a child (yes, no); Indicator: a participant has his or her own mobile phone; Indicator: number of sex partners in the last 12 months (zero, one, more than one); Indicator: time of the most recent HIV test (never tested, tested more than a year ago, tested less than a year ago); Age: in years; Education: highest grade completed Knowledge about HIV; Number of correct answers to the 15 questions testing HIV-related knowledge; Subjective risk of HIV infection (coded 1 to 5); The straight-line distance between the household and the testing clinic (in km); Square of the straight-line distance between the household and the testing clinic (in km); Indicator: the household ever go without food in the last 12 months (yes, no); Indicator: there is an HIV positive household member (yes, no); Asset ownership index: the first principal component of 14 asset-ownership indicators; Enumerator fixed-effects; Community fixed-effects. If any missing value exists for some variable “X,” an indicator variable is created for variable X to flag missing status (1 if missing, 0 otherwise). The missing value of the variable X is replaced with zero. The variable X missing indicator variable is added to the set of control variables.

In conclusion, social stigma concerns attached to HIV is a barrier to the test uptake for those people who overestimate stigma. Learning the evidence of a low-stigma local environment raised the likelihood that the concerned individual would take a test by 7.7 percentage points, or by 37%.

<sup>15</sup>Equation (1.1) is equivalent to the primary regression equation specified in the Pre-Analysis Plan in identifying intervention effects. I present the regression analysis of Equation (1.1) in this paper for a more intuitive interpretation. Definitions of control variables  $\mathbb{X}_i$  and sample inclusion criteria used for Equation (1.1) followed the Pre-Analysis Plan. Conducting the primary regression specified in the Pre-Analysis Plan reaches qualitatively the same and quantitatively very similar conclusions. See Appendix C for Details.



### 1.4.2 Quantifying the Intervention Effect

To quantify the stigma barrier, I include the High-Incentive Group in the analysis. Figure 1.6 adds the test uptake rate of the High-Incentive Group in Figure 1.5. Table 1.4 Column (1) presents the regression analog. Column (2) presents the regression coefficient for Equation (1.2):

$$Y_i = \alpha + \beta_1 G_i^{relieve} + \beta_2 G_i^{high-incent} + \mathbb{X}_i + \epsilon_i. \quad (1.2)$$

$G_i^{high-incent}$  is the indicator of receiving 100-Metical coupons as opposed to 50-Metical ones. Doubling the monetary incentive raises the test uptake by 12.0 percentage points.

Participants from the Control Group and the High-Incentive Group are under the same experimental condition (not exposed to concern-relieving intervention) but have received different monetary incentives to take an HIV test. The varying incentive value for the HIV test allows us to locally pin down the demand curve for an HIV test. I derived the demand curve in Figure 1.7 with point estimates from Table 1.4, Column (2). Consider the monetary incentives for taking a test as negative prices. At the price of -50 Meticaïs, the test uptake rate is 20.7 percent. Lowering the price to -100 Meticaïs increases the test rate by 12.0 percentage points and reaches 32.7 percent. Keeping the price at -50 Meticaïs but having the excessive stigma concern corrected increases the test rate by 6.9 percentage points, reaching 27.6%. Assuming local linearity, the concern-relieving intervention leads to a 29-Metical (1.30 dollars by PPP) increase in willingness to pay (WTP) for an HIV test. The size of the increase is over half of the daily cost-of-living.

One caveat when interpreting the change of WTP induced by the concern-relieving intervention is that it is only valid around the price levels on which we conducted experiments (between negative 50 to negative 100 Meticaïs). We remain agnostic about the demand curve's shape beyond this price range. Researchers have pointed out that a financial incentive itself may relieve some of the stigma concerns by allowing test takers to conceal their real motivations for taking the test (Thornton, 2008; Swann, 2018). If that is the case, then the demand curve for an HIV test will jump discontinuously at the price equal to zero. The effect of the concern-relieving intervention on the WTP at a positive price can differ from that at a negative price. However, since both the Control Group and the intervention group received the same financial

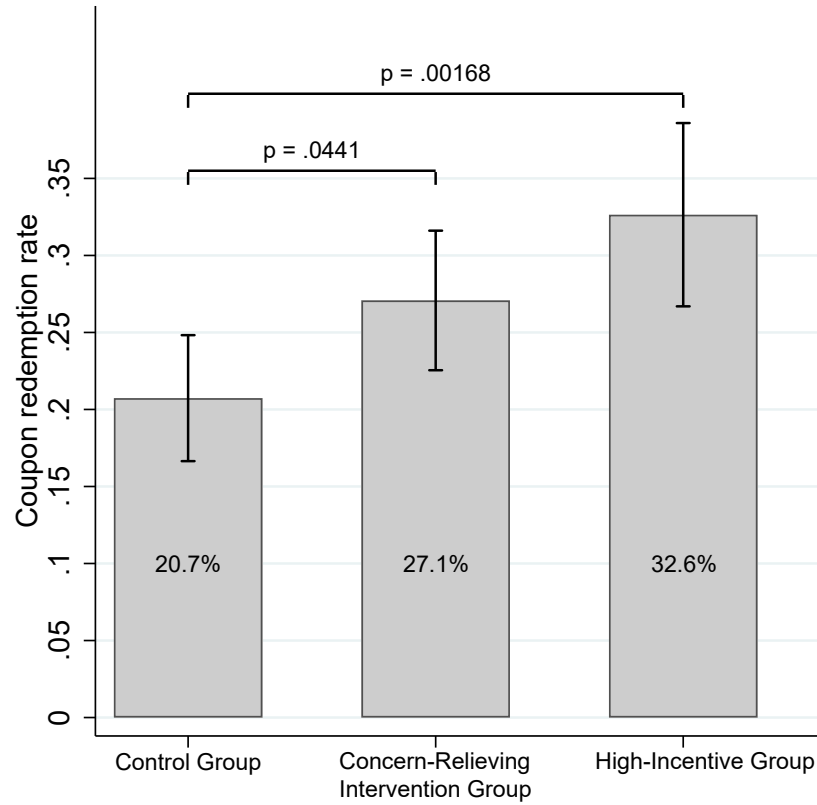


Figure 1.6: Test Uptake Mean Comparison: Three Study Groups

**Notes:** The *y-axis* is the HIV test uptake rate measured by the fraction of participants redeemed testing coupons. Both the Concern-Relieving Intervention Group and the Control Group received coupons of the value of 50 Meticaïs. The High-Incentive Group received coupons of the value of 100 Meticaïs.

Table 1.4: Quantifying the Intervention Effect

Group Indicators	(1)	(2)
	Test Uptake	
Concern-Relieving Intervention	0.0634** (0.0315)	0.0686** (0.0323)
High-Incentive	0.119*** (0.0377)	0.120*** (0.0389)
Control Group Mean	0.207	0.207
Observations	996	996
R-squared	0.011	0.247
Constant	yes	yes
Controls	no	yes

**Notes:** Standard errors in parentheses. Standard errors are clustered at the household level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

The control variables are the same as in Table 1.3.

incentive, the design in this study is always valid in identifying the *existence* of the stigma-concern barrier to testing, regardless of the financial incentive’s effect.

### 1.4.3 Heterogeneity by Belief Update

The concern relieving treatment is informational by nature. It is designed to change an individual’s behavior by first changing his or her perceptions. An immediate inference is that the intervention will show the strongest effect on people who are open to and able to perceive the new information. Below I present suggestive evidence that this is the case.

We introduced a “concern re-assess” procedure to a subset of the concern-relieving group. The “concern re-assess” applied to a participant 15 minutes after the intervention was performed. (During the interval between the treatment and the re-assess, the participant was occupied by answering other survey questions unrelated to health or HIV.) In the “concern re-assess” session, the enumerator re-asked the questions in which the participant overestimated the stigma at first and learned the correct answers during the intervention. A participant could still give an answer suggesting high stigma concerns in the re-assess session, either due to lack of trust in the enumerator-shared information or due to the inability to process and remember the

information. If a participant still overestimated stigma in the re-assess session, the enumerator would repeat the intervention one more time.

The “concern re-assess” allows us to observe participants’ updates of beliefs in the immediate short term. Participants updating belief in the right direction were those who took the new information seriously and correctly. We call them “fast-updaters.” Due to resource constraints, the re-assess procedure was implemented only in the provinces of Zambezia and Sofala. Two-thirds of the participants in the re-assess session updated their beliefs about stigma in the correct direction.

Regressions in Table 1.5 explore intervention heterogeneity between the fast-updaters and others. Column (1) replicates Column (2) of Table 1.3 to show the main intervention effect. Column (2) and Column (3) run the same regression separately on two subsamples: the one that we did not conduct a “concern re-assess” (Manica province) and the subsample that we did (Zambezia province and Sofala province). The two subsamples present very similar main intervention effects. Column (4) runs a regression that includes the interaction between concern-relieving intervention and fast-updater status. Since we were not able to measure whether a participant from the Control Group would be a “fast updater”; an individual’s ability to digest information and update beliefs is not fully controlled for. The point estimate of the main effect shrinks to zero, and the intervention effect on the fast-updaters is almost twice the size of the main effect in Column (3). Table 1.5 strongly supports the inference that the concern-relieving intervention is more effective on the fast updaters. (In fact, the main effect is entirely driven by them.)

Table 1.6 further explores intervention heterogeneity by four dimensions: gender, education level, wealth, and subjective risk of infection. I split the sample into two subgroups by each of the dimensions and run Equation (1.2). Coefficients obtained from the high-education subsample remain significant after adjusting for Multiple Hypotheses Testing (List, Shaikh and Xu, 2019). Table 1.6 also reports test of equality. The effect of the concern-relieving intervention exhibits strong heterogeneity by education levels: It is close to zero in the low-education group in contrast to 16.0 percentage points in the high-education one.

Table 1.5: Heterogeneous Intervention Effect on Test Uptake by Belief Updates

	(1)	(2)	(3)	(4)
Sample	Full sample	Not Re-assessed Sample	Re-assessed Sample	Re-assess sample
Concern-Relieving Intervention	0.0771** (0.0326)	0.0766* (0.0436)	0.0895* (0.0539)	-0.0272 (0.0687)
Concern-Relieving Intervention × Fast updater				0.166** (0.0800)
Control Group Mean	0.207	0.181	0.241	0.241
Observations	754	413	341	341
R-squared	0.292	0.241	0.383	0.392
Constant	yes	yes	yes	yes
Controls	yes	yes	yes	yes

**Notes:** Standard errors in parentheses. Standard errors are clustered at the household level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

The control variables are the same as in Table 1.3.

Table 1.6: Heterogeneous Intervention Effect on Test Uptake Across Subgroups

Subgroup	(1) Male	(2) Female	(3) Low educ	(4) High educ	(5) Poor	(6) Wealthy	(7) Low subrisk	(8) High subrisk
Concern-Relieving Intervention ( $\beta_1$ )	0.0886 (0.0703)	0.0495 (0.0402)	-0.0148 (0.0550)	0.160*** (0.0468)	0.0197 (0.0490)	0.112*** (0.0532)	0.0996* (0.0509)	0.00944 (0.0583)
High-Incentive ( $\beta_2$ )	0.162*** (0.0821)	0.114*** (0.0517)	0.0925 (0.0595)	0.181*** (0.0594)	0.124*** (0.0590)	0.103 (0.0654)	0.140*** (0.0616)	0.0395 (0.0680)
Control Group Mean	0.175	0.222	0.279	0.156	0.226	0.190	0.233	0.190
LSX adjusted p-value: $\beta_1$	0.814	0.797	0.962	0.029	0.977	0.422	0.487	0.884
LSX adjusted p-value: $\beta_2$	0.599	0.423	0.786	0.062	0.559	0.654	0.352	0.970
Test of equality p-value: $\beta_1$	0.541		0.004			0.129		0.166
Test of equality p-value: $\beta_2$	0.531		0.207			0.780		0.195
Observations	324	672	446	545	498	498	523	442
R-squared	0.016	0.003	0.001	0.025	0.005	0.006	0.003	0.008
Constant	yes	yes	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes	yes	yes

**Notes:** Robust standard errors in parentheses. Standard errors are clustered at the household level. Stars attached to the coefficients reflect unadjusted p-values: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . This table also reports p-values that are adjusted for multiple hypotheses testing (16 tests in total, 2 coefficients by 8 subgroups) according to List, Shaikh and Xu (2019). Each pair of the intervention-effect coefficients are tested against equality. p-values of equality tests are reported. “Low educ” group has participants with below-median years of education (5 years or less); “High educ” group with above-median years of education (6 years or more). “Poor”/“Wealthy” status are determined by the first principle components of ownership of 14 assets. The cut off value is 0.250. Participants with low subjective risk of infection (“Low subsrisk”) are those who believed themselves to be HIV negative. “High subsrisk” group are participants with low subjective risk of infection (“Low subsrisk”) are those who believed themselves to be HIV negative. “High subsrisk” group are participants with low subjective risk of infection (“Low subsrisk”) are those who believed themselves to be HIV negative. In the regressions with controls, the control variables are the same as those in 1.3, while Column (1) (2) drop female indicator, Column (3) (4) drop education level, Column (5) (6) drop asset indicator, Column (7) (8) drop subject risk index.

Individuals with more education responded stronger to the concern-relieving intervention potentially because they were able to process the information better. This finding is consistent with the fact that the intervention is more effective on the fast updaters. Both dimensions of heterogeneity remind us that when applying informational experiments to a low-literacy population, participants' ability to understand the information can substantially affect the intervention's impacts.

#### 1.4.4 Stigma and Test Uptake of Children

Till now, we have focused on adult participants and found that their stigma concerns have discouraged their test-seeking behavior. 71% of the adult participants are parents and make decisions for their children when it comes to HIV testing. If stigma concerns hold adults back from HIV tests for themselves, do they hold children back, too? The potential inter-generational effect of stigma concerns has important implications because a child's early experience in HIV testing can have prolonged effects not only on their short-term health status but also on their future habits about and attitude towards health behavior when they reach adulthood. This study can show the role of stigma concerns on children's test-seeking behavior with its children's coupon design.

At the stage of coupon distribution at the participant's home, each eligible child of the survey respondent was offered coupons to take an HIV test, regardless of the eligibility of the adult respondent. Each child received a coupon of the same value as the adult household members. Children's coupons were handed to the parent who answered questions on their behalf. In the analyses below, a child's group assignment is considered the same as the parent who answered questions on their behalf. We did not directly interact with children; in the Concern-Relieving Intervention Group, only adults received the intervention.

In Table 1.7 we report regressions similar to Table 1.4, Column (2), but with a sample of children. A child enters the regression in Column (1) if one of her parents overestimated stigma, and the child herself is eligible to receive a coupon for testing.

The child sample presents a test uptake rate of 30.1% under the control condition, higher than that of the adult sample. The high financial incentive displays a similar impact, 9.3 percentage points, on the child testing as on the adult testing. The concern-relieving intervention, on the other hand, does not play a significant role in raising the test uptake rate in children: in Table 1.7 Column (1), the point estimate

Table 1.7: Concern-Relieving Intervention Effect on Children

Variables	(1)	(2)
	Test Uptake	
Concern-Relieving Intervention	0.0147 (0.0301)	0.0298 (0.0366)
High-Incentive	0.0934** (0.0363)	0.137*** (0.0434)
Parent tested within 3 months		0.126** (0.0599)
Parent HIV positive		0.0538 (0.0847)
Concern-Relieving Intervention × Parent tested within 3 months		-0.0905 (0.0789)
Concern-Relieving Intervention × Parent HIV positive		0.0290 (0.0944)
High-Incentive × Parent tested within 3 months		-0.171* (0.0925)
High-Incentive × Parent HIV positive		-0.0618 (0.103)
Control Group Mean	0.301	0.301
Observations	3,519	3,519
R-squared	0.215	0.220
Constant	yes	yes
Controls	yes	yes

**Notes:** Standard errors in parentheses. Standard errors are clustered at the household level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

The control variables are: Indicator: a child is female (yes, no); Indicator: time of the most recent HIV test (never tested, tested more than a year ago, tested less than a year ago); Age: in years; Indicator: the participating parent is female (yes, no); Parent's Education: highest grade completed; Parent's Knowledge about HIV: Number of correct answers to the 15 questions testing HIV-related knowledge; The straight-line distance between the household and the testing clinic (in km); Square of the straight-line distance between the household and the testing clinic (in km); Indicator: the household ever go without food in the last 12 months (yes, no); Indicator: there is an HIV positive household member (yes, no); Asset ownership index: the first principal component of 14 asset-ownership indicators; Enumerator fixed-effects; Community fixed-effects. If any missing value exists for some variable "X," an indicator variable is created for variable X to flag missing status (1 if missing, 0 otherwise). The missing value of the variable X is replaced with zero. The variable X missing indicator variable is added to the set of control variables.



of the intervention effect is 1.5 percentage points. Table 1.7 Column (2) examines how our interventions interact with parents' testing history (the parent was eligible for a coupon, the parent had been tested within 3 months, or the parent had been tested HIV positive). When the parent him or herself was eligible for a coupon, the concern-relieving intervention raises children's test uptake by 3.0 percentage point and the effect is still not statistically different from zero. Children's testing behavior is strongly correlated with their parents'. Parents who voluntarily sought tests for themselves within 3 months before our study are 12.6 percentage points more likely to take their children to be tested with our coupons. Both the concern-relieving intervention and the high incentives appear to be substitutes for a parent's active testing history. The interaction term between the parents tested within 3 months and either the intervention or the high incentives is negative and sizable.

Although we find that children's testing behavior is closely correlated with their parents', a parent's stigma concerns do not appear to be a major barrier when it comes to children's test uptake. The high test uptake rate among children in the Control Group and lack of effect of the concern-relieving intervention provide suggestive evidence. That the stigma barrier plays a less important role in children's testing than adults' testing may stem from the nature of the stigma. HIV infection is associated with some socially disapproved adult behaviors but not child behaviors; thus, parents believed that children are less prone to stigmatization. Another possible explanation is that even though children are equally prone to stigmatization, only the health status of children, but not their social relationship concerns, enters a parent's utility function. The experiment design in this study does not allow us to separate these different explanations. Learning stigma concerns' role in children's test uptake requires future work.

#### **1.4.5 Demand for Testing among the Concerned and the Unconcerned**

The analyses above have focused on the population that had excessive stigma concerns. We have shown that the concern-relieving intervention encouraged these individuals to take an HIV test and that the effect size is comparable to that of doubling the financial incentives. We also depicted their demand for HIV tests in the short term (14 days) in Figure 1.7 and discussed how the intervention effect fit into the demand curve.

In this section, we gauge the concern-relieving intervention from a different angle. Specifically, we try to answer the following questions: To what degree has my inter-

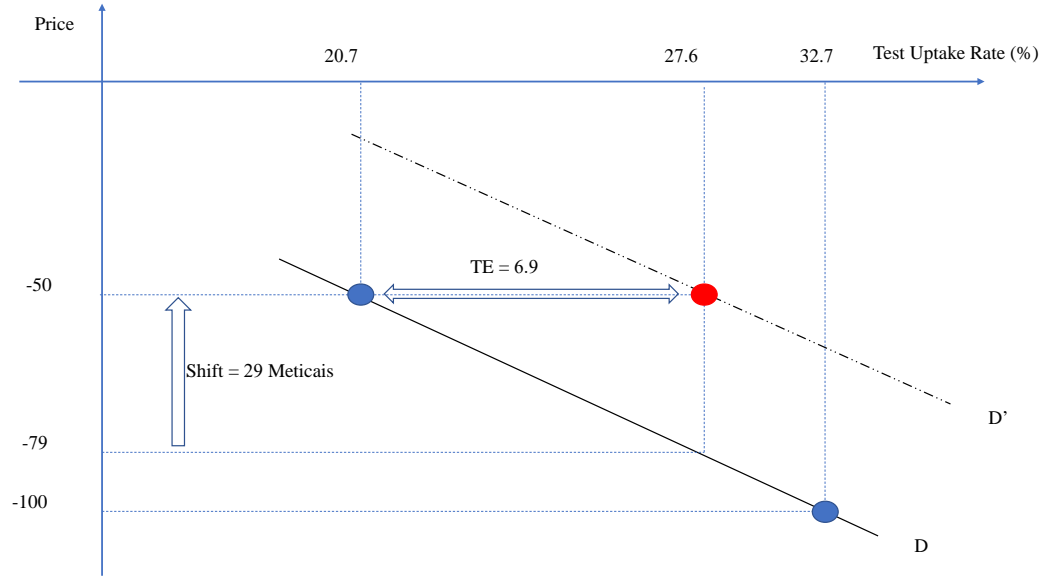


Figure 1.7: Quantifying the Intervention Effect on HIV Test Demand Curve

**Notes:** Curve D depicts the demand for an HIV test within 14 days among the “concerned” population. This study experimentally sets two price levels for a test: negative 50 Meticaais (receive 50 Meticaais conditional testing) and negative 100 Meticaais (receive 100 Meticaais conditional testing). The *x-axis* of this demand diagram is the percent of people that take the test.

vention helped “concerned” individuals catch up with the “unconcerned” individuals in taking HIV tests?

A naive approach is to compare the test uptake rate measured by coupon redemption of the Concern-Relieving Intervention Group (27.1%) with that of the “unconcerned” participants with 50-metical coupons (23.3%). This comparison would lead to the conclusion that the test uptake rate of the “concerned” after receiving the concern-relieving intervention will surpass that of the “unconcerned” people by 3.8 percentage points. This approach is biased, however, because the two groups are not comparable.<sup>16</sup> The “unconcerned” were more likely to have already tested shortly before the study, and as a result, were ineligible for coupons. Those who were “unconcerned” and received coupons was a sample that faced higher barriers beyond stigma concerns.

To get a comparable sample, consider everyone who did not know they were HIV positive at the time of 3 months before the study. We calculate their demand for an HIV test in the next three and a half months. Every one of them had the chance to

<sup>16</sup>See Appendix Table A.2 for comparisons between the concerned and unconcerned participants.

take an HIV test without any financial incentives in the next 3 months, and if they chose not to, they received coupons from this study to take tests in the following 14 days.

To simplify the analysis, for now, we assume that learning one's HIV status does not affect one's belief about stigma in society. Thus, the "concern" status obtained from the study survey correctly reflects participants' "concern" status 3 months ago. We will later discuss the implications of our results if the assumption does not hold.

Table 1.8 summarizes the choices in the next three and half months of those who did not know they were HIV positive at the time of 3 months before the study, separately for the "concerned" and "unconcerned." 28.4% of the concerned and 31.3% of the unconcerned chose to take a test in the next 3 months. Those who did not take a test (71.6% of the concerned and 68.7% of the unconcerned) received coupons from this study. Of those who received coupons, when the coupon value is 50 Meticaïs, 20.7% of the concerned and 23.3% of the unconcerned took a test. When the intervention is applied in addition to the 50-Metical coupons, 27.1% of the concerned people took a test.

Combining the numbers, the fraction of concerned people who would have taken a test in the 3.5 months at the price of negative 50 is  $28.4\% + (1-28.4\%) \times 20.1\% = 43.2\%$ . Similarly, the demand for a test in the 3.5 months period for the unconcerned group is  $31.3\% + (1-31.3\%) \times 23.3\% = 47.3\%$  at the price of negative. Of the concerned people who also received our intervention and 50-Metical coupons, the test uptake rate within the 3.5 months period would be  $28.4\% + (1-28.4\%) \times 27.1\% = 47.8\%$ . The relationships are depicted in Figure 1.8. In conclusion, when we examine a 3.5-month period, the concern-relieving intervention makes the people concerned with stigma catch up with, and even surpass, those who were unconcerned.

Now consider how violations of the assumption would affect this result. If learning one's HIV status makes people believe that there is less stigma, then some people who tested for HIV before the study (row (1) of Table 1.8) and were assessed as "unconcerned" in the study survey were in fact "concerned" 3 months before the study. That means the test uptake rate within 3 months before the study for the "concerned" should be higher than 28.4%, while, for the "unconcerned," it should be lower than 31.3%. As a result, with the intervention, the 3.5-months test uptake rate of the concerned should be higher than 47.8% and that of the unconcerned lower than 47.3%. Correcting this bias would suggest that a previously "concerned" person with

Table 1.8: Test Uptake of the Concerned and Unconcerned

<b>Panel A: Concerned</b>		
(1): Test uptake rate within 3 months before the RCT		28.4%
(2) = 100% - (1): Entered the RCT		71.6%
	Without intervention	With intervention
<i>(3): Test uptake rate with 50-Metical coupons 14 days into the RCT</i>	20.7%	27.1%
Test uptake rate during the 3.5 months at price = -50 Meticaïs	43.2%	47.8%
(1) + (2)*(3)		
<b>Panel B: Unconcerned</b>		
(4): Test uptake rate within 3 months before the RCT		31.3%
(5) = 100% - (4): Entered the RCT		68.7%
	Without intervention	With intervention
<i>(6): Test uptake rate with 50-Metical coupons 14 days into the RCT</i>	23.3%	-
Test uptake rate during the 3.5 months at price = -50 Meticaïs	47.3%	-
(4) + (5)*(6)		

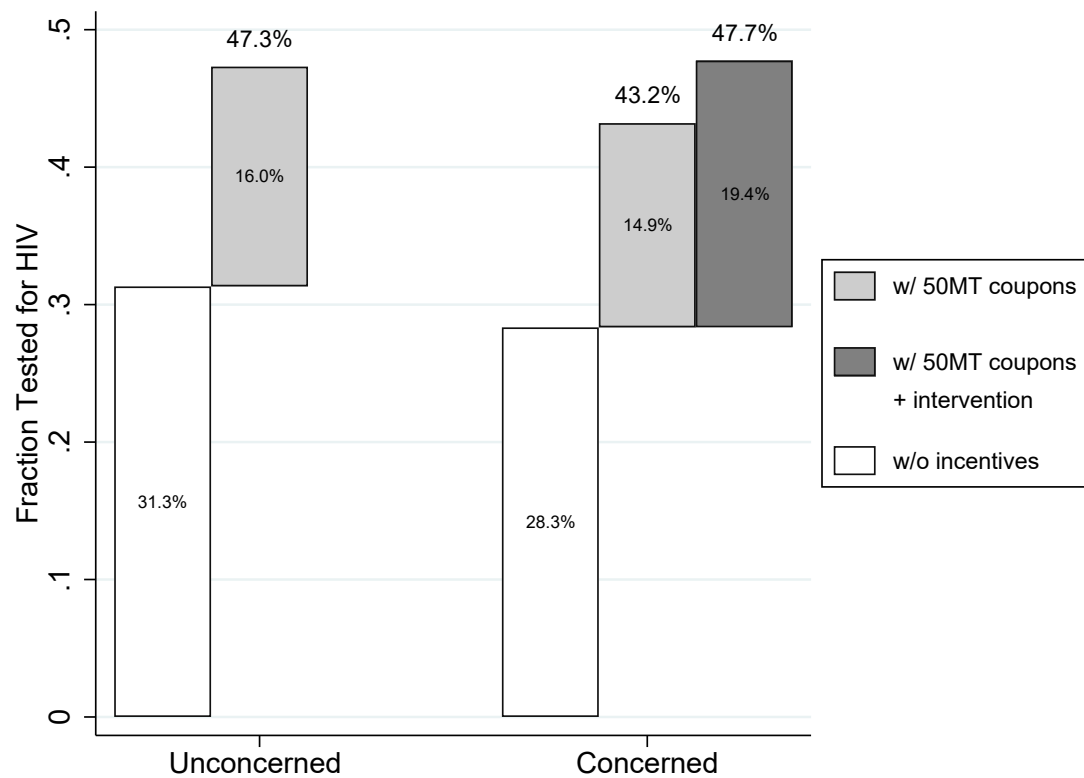


Figure 1.8: Demand for Testing among the Concerned and Unconcerned

**Notes:** The analysis sample of this figure is all survey respondents that were eligible for a coupon or were tested for HIV within 3 months before the recruitment survey. The *y-axis* is the HIV test uptake rate.

the concern-relieving treatment became even more likely to take an incentivized HIV test than an “unconcerned” person.

## 1.5 Conclusion

This paper analyzed a randomized control trial to identify the role of stigma concerns in hindering HIV testing and to quantify the stigma barrier.

We obtained local stigma environment measures of the study communities one year before the RCT and used these measures to construct an intervention to experimentally mitigate individuals’ concerns for stigma. Participants with excessive stigma concerns were randomized to receive the concern-relieving intervention; in this intervention they are informed of the true stigma environment measures of their communities that suggested lower-than-expected stigma. We then tracked participants’ test-seeking behavior with testing coupons.

This paper first establishes evidence that the stigma concerns are a barrier that has caused people to avoid taking an HIV test. Participants from the Concern-Relieving Intervention Group took up HIV tests 7.7 percentage points more or by 37% more, than those from the Control Group.

Moreover, the experiment design allowed us to give the stigma barrier a dollar value. We introduced study groups with different levels of monetary incentives for HIV testing. The Control Group and the Concern-Relieving Intervention Group received coupons of 50 Meticaïs (2.25 dollars by PPP, equivalent to the daily cost-of-living). In an additional study group, the High-Incentive Group, participants received no intervention but coupons of 100 Meticaïs. The Control Group and the High-Incentive Group locally pin down the demand curve for HIV testing. In the demand framework, the concern-relieving intervention raised individuals’ willingness to pay (WTP) for an HIV test by 29 Meticaïs (1.30 dollars by PPP, or more than half of the daily cost-of-living).

This paper conducted additional analyses to depict the role of stigma concerns in HIV testing. Our concern-relieving intervention is most effective on those who were able to perceive the information immediately or those with more years of educations, which suggests that participants’ capacity to process new information substantially affects the success of this informational intervention. In exploring children’s behavior, the study shows that children’s test uptake rate under the control condition is higher than that of their parents’ and that stigma concerns do not appear to play a significant

role when parents make test-seeking decisions for their children. Combining self-reported history with the coupon redemption, we found that the simple concern-relieving intervention is effective enough to help individuals with excessive concerns take HIV tests at a similar rate as those in the “unconcerned” group.

In response to the HIV epidemic, global donors and national governments have launched a wide variety of campaigns in Sub-Saharan Africa. Many of these contain informational components that disseminate knowledge about the disease and promote supportive attitudes towards the infected population. This paper suggests a new piece of information, the social stigma measures, that holds much promise in promoting healthy behavior regarding HIV prevention and treatment. Building on the fact that the stigma environment has continuously improved in Sub-Saharan Africa, I showed that letting people learn the supportive, low-stigma environment of their community has a large positive impact on the public uptake of HIV tests. This paper calls for policymakers to pay particular attention to the social stigma barrier when advancing public health programs related to HIV. The informational intervention designed in this study can be scaled up in a broader population at a reasonable cost and can fit into campaigns fighting HIV.

## CHAPTER II

# HIV Testing, Knowledge, and Stigma: An Analysis of a Widespread HIV/AIDS Program

### 2.1 Introduction

National governments and the global development community have pursued a wide variety of programs to combat the HIV/AIDS pandemic. Across the broad scope of such programs, efforts to facilitate and promote HIV testing play a central role. HIV testing is a central focus because, first of all, testing is the prerequisite for being diagnosed as HIV positive and thereafter initiating life-saving anti-retroviral therapy. Furthermore, HIV is typically asymptomatic for years before the disease progresses to AIDS and symptoms become apparent. During the asymptomatic phase of the disease, HIV testing leading to treatment via antiretroviral therapy (ART) at this early stage has substantial benefits on two key dimensions. First, treatment leads to lower viral loads and thus a much lower likelihood of transmission to sexual partners (Cohen et al., 2013). Second, an HIV-positive individual's longer-run adherence to treatment and health outcomes are better when treatment occurs at an earlier stage of the disease (Ford et al., 2018).

There has been substantial progress in implementing HIV testing around the world, particularly in Sub-Saharan Africa where HIV prevalence remains the highest on the planet. That said, testing rates remain quite far from optimal levels for control of the epidemic. In our study population, nearly half of adults and almost 90% of children have never been tested for HIV. There thus remains a great deal of room for improvement in HIV testing rates.

We seek to shed light on the impact of a major type of program combating HIV/AIDS on HIV testing rates, and to understand the mechanisms underlying its



effectiveness (or lack thereof). We focus on two mechanisms in particular: alleviating imperfect information related to HIV, and reducing HIV-related stigmatizing attitudes. We study a program in Mozambique, *Força à Comunidade e Crianças* (FCC, “Strengthening Communities and Children”), funded by the U.S. government’s Presidential Emergency Plan for AIDS Relief (PEPFAR), that aims to raise HIV testing rates by improving households’ information about HIV/AIDS and reducing HIV-related stigmatizing attitudes. FCC is a community-level program that operates primarily via home visits to households, alongside a set of complementary interventions in communities and schools. The program is representative of a broad category of PEPFAR-funded interventions to help households and communities respond to the HIV/AIDS crisis, known as programs for “orphans and vulnerable children” (OVCs).

We exploit random assignment of household exposure to the FCC program to: 1) estimate the causal impact of such exposure on household HIV testing rates, and 2) reveal whether any such improvements operate via improvements in HIV-related information and reductions in HIV-related stigmatizing attitudes. Our approach involves a three-part randomized controlled trial methodology. First, communities as a whole were randomly assigned to treatment or control status (inclusion in or exclusion from the FCC program). Second, a subset of households within treatment communities were randomly assigned to a strong encouragement to participate in FCC programs ( “directly enrolled” households). These directly enrolled households receive a home visit by an FCC program community worker and are assessed for inclusion in various FCC subcomponent programs. This led them to have higher participation rates in the program than other households in treatment communities. Other households not randomly selected for direct enrollment ended up being treated as well, but at lower rates. These first two randomization components allow us to shed light on direct impacts, and to quantify spillovers within treatment communities from directly-enrolled to other households.

The third part of the randomized methodology is aimed at shedding light on the role of particular mechanisms behind impacts of the FCC program: information about HIV, information about HIV treatment, ART, and reducing concerns about HIV-related stigma. We randomly assign simple treatments at the household level that our project staff administer immediately after the endline survey. To gauge effect magnitudes, we also randomly assign the size of monetary incentives for HIV testing. We examine how these treatments affect one of our HIV testing outcomes, use of a coupon for HIV testing after the endline survey.

As specified in a pre-analysis plan (PAP), our primary analyses focus on the effect of a household being assigned to “direct enrollment” in a treatment community, compared to all households in control communities. We conduct our analyses in a sample of 4,240 households in 76 Mozambican communities who we have been following through a 2017-18 baseline and 2019 endline survey. The primary outcome of interest is a composite outcome equal to one if anyone in a household is reported to have had an HIV test in the last 12 months (reported in the endline survey), or if anyone in the household uses a coupon for HIV testing at the local health clinic, provided by our research staff during the endline survey, within the next 14 days; and zero otherwise. Secondary outcomes, including those used to measure information and stigma mechanisms, are from the endline survey.

We find that the FCC program has positive effects on the composite measure of HIV testing (the combination of the 12-month self-report and the post-endline-survey coupon use), but these effects are small: an increase of 2.21 percentage points, on top of a base of 72.3 percent in the control group. The effect is also only marginally statistically significantly different from zero (p-value 0.263). The treatment effect is similar for each component of the testing measure examined separately: 2.45 percentage points for the 12-month self-report (control mean 62.6 percent; p-value 0.315), and -2.18 percentage points for testing coupon use (control mean 26.4 percent; p-value 0.226).

A useful way to view the modest size of this treatment effect is to compare it to expert predictions of the effect size. Prior to our results being known, in DellaVigna, Otis and Vivaldi (2020) collected from subject-matter experts their forecasts of the treatment effect of being assigned to “direct enrollment” in a treatment community on the 12-month self-report of HIV testing. The mean expert prediction was 11.36 percentage points. Our actual treatment effect, 2.45 percentage points, is substantially below the expert prediction: it is only one-fifth the magnitude of the expert prediction, and the difference between the two is highly statistically significant at conventional levels (p-value 0.0004).

The program has no positive impact on other pre-specified secondary outcomes, such as school enrollment and attendance, assets, life satisfaction, and adherence to ART. We also find no evidence of HIV testing spillovers (via geographic or social proximity) from directly-enrolled to other households in treatment communities (also pre-specified).

We then turn to understanding mechanisms behind the FCC program’s modest effects on HIV testing. In secondary analyses that we also pre-specified, we examine knowledge about HIV, HIV-related stigmatizing attitudes, and safe sexual behavior. The program did not improve HIV-related knowledge, as measured by the share of correctly-answered responses to 33 questions. Strikingly, the treatment actually worsened HIV-related stigmatizing attitudes, measured by the share of non-stigmatizing answers to four widely-used questions. Finally, the treatment led to improvements in an index of self-reported safe sexual behavior, and reductions in the number of sexual partners in the last 12 months.

The additional treatments randomly assigned at the household level during our follow-up survey provide further highlight the role of stigma and information. We find that separate treatments providing HIV-related information and alleviating concerns about HIV-related stigma raise the impact of the FCC program on HIV testing. These impacts on increasing the impact of the FCC program on testing are comparable in size to the impact of a financial incentive for HIV testing about the size of the average daily wage. These findings help confirm that the FCC program was deficient in the areas of improving HIV knowledge and reducing stigmatizing attitudes.

We use the following conceptual framework to tie these different results together. People decide whether to get tested for HIV, assess the costs and benefits of getting tested, and get tested if the perceived benefits exceed perceived costs. FCC program seeks to foster HIV testing by raising the perceived benefit of testing and reducing perceived costs. It does so by implementing home visits to households (alongside other complementary interventions) to improve knowledge about HIV, and reducing HIV-related stigmatizing attitudes in the community. Improved knowledge about HIV raises the perceived benefit of testing, while reducing stigmatizing attitudes reduces the perceived cost of testing. In addition, improved knowledge about HIV also raises testing indirectly since it may also reduce stigmatizing attitudes, which then further increases testing.

With knowledge having such a central role in raising HIV testing, much can go wrong if a program such as FCC inadvertently fails in its knowledge-raising objective, and instead creates misinformation. Our results are consistent with the FCC program actually worsening knowledge, which has a direct effect on reducing testing, as well as an indirect effect via increasing stigmatizing attitudes, which further reduces testing. This interpretation of the results also helps explain the increase in safe sexual behaviors, in particular reductions in the number of recent sexual partners. Increased

fear of being stigmatized could cause people to practice safer sexual practices, so as to avoid HIV infection and the associated stigma.

This research is connected to a number of research areas in economics and public health. In the context of studies of community-level interventions to combat the HIV/AIDS crisis, the use of randomized controlled trial methodologies is rare. Prior studies of PEPFAR programs have not exploited prospectively randomized research designs, and instead have relied on retrospective analysis with control or comparison groups that were not randomly selected. Relatedly, past studies have not tracked defined groups of individuals over time (from before to after program implementation), raising additional concerns about sample selection biases (Bryant et al., 2012). Ben-david et al. (2012) examine the impact of PEPFAR funding at the country level using a difference-in-difference approach, finding substantial reductions in adult mortality in Africa. A number of past studies have used randomized controlled trials to examine the impact of narrower, more targeted interventions related to HIV/AIDS, such as Thornton (2008), McCoy et al. (2017), Ssewamala, Han and Neilands (2009), Ivers et al. (2014), Baird, McIntosh and Ozler (2011), Kiene et al. (2017), and Yotebieng et al. (2017). None of these have studied community-level programs, or examined the interplay between testing, knowledge, and stigmatizing attitudes as we do.

It is important to understand the extent to which various programs raise rates of HIV testing, but it is just as important to understand the mechanisms through which they do so. An understanding of mechanisms can shed light on the underlying market failures, and thereby help guide future policy in the HIV/AIDS realm (and potentially other related policy areas). We study whether improvements in information in beneficiary households are an important mechanism through which direct and spillover effects of such programs operate. Our findings can motivate future policies to address informational market failures (and future research to delve more deeply into them). Another mechanism through which program effects may operate is via reductions in HIV-related stigmatizing attitudes in the social network. We find increases in stigmatizing attitudes, which may be behind reduced willingness to be tested for HIV. The role of stigma in inhibiting health care utilization has not been widely studied in economics, in the HIV/AIDS context or elsewhere.

## 2.2 Conceptual Matters

Among programs that have the promotion of HIV testing as a primary aim, a common theory of change is as follows. People decide whether to get tested for HIV assess the costs and benefits of getting tested, and get tested if the perceived benefits exceed perceived costs. Perhaps the most important benefit of getting tested is that if testing positive, one can then initiate treatment, anti-retroviral therapy (ART). Initiating ART early leads to better health outcomes for the HIV-infected person, and has spillovers onto others by reducing transmission. On the cost side, HIV testing may itself be free of monetary costs (as in our context), but it still involves time and effort costs. There are also social costs of experiencing HIV-related stigmatizing attitudes from others, as well as potential psychological costs such as anxiety and distress that can come from learning of a positive test result.

We view the FCC program we study as seeking to raise rates of HIV testing by raising the perceived benefit of testing and reducing perceived costs. It does so by implementing home visits to households (alongside other complementary interventions) to improve knowledge about HIV, and reducing HIV-related stigmatizing attitudes in the community.

Imperfect information on HIV and HIV treatment leads households to underestimate the potential benefits of HIV testing. Improving HIV-related knowledge may raise the perceived benefits of getting tested. This should then increase rates of HIV testing as households become more likely to judge that the benefits of testing to outweigh the costs.

Concerns about HIV-related stigmatizing attitudes raise the perceived costs of HIV testing. Stigmatizing attitudes towards people infected with HIV impose social and psychic costs on them, as well as on people suspected to be infected with HIV. HIV testing is intended to be anonymous, but individuals may be concerned that their anonymity may not be fully protected if they go for testing. Travel to a health clinic can be observed by others, and one's presence in a clinic can be observed by other patients. In contexts where high shares of health services at clinics are related to HIV testing and treatment, those observing someone traveling to or being in a health clinic may place some positive probability in their being infected with HIV. Moreover, individuals may not trust healthcare workers to keep secret that they came for testing, or to be discreet about their HIV status. An intervention that reduces stigmatizing attitudes may raise HIV testing rates by reducing the perceived costs

of going for testing, making it more likely that individuals judge that the benefits of testing exceed the costs.

The FCC program may reduce HIV-related stigmatizing attitudes in two ways. First, community workers doing home visits are expected to engage households in “sensitization” conversations to try to reduce stigmatizing attitudes and foster more accepting and supportive attitudes towards HIV-infected individuals.

Second, simply improving knowledge about HIV as a disease and how it is treated could reduce stigmatizing attitudes. If people learn that HIV can only be spread via sharing of bodily fluids (and not through casual contact), this could make people more comfortable interacting with HIV-infected individuals, which we would view as a reduction in stigma. Stigmatizing attitudes could also fall as people learn that HIV is a treatable disease, treatment is provided for free at the local health clinic, people under treatment can lead relatively normal lives. In other words, if the FCC program is successful in improving knowledge about HIV, it could also succeed in reducing stigmatizing attitudes.

In this simple framework, efforts to improve HIV-related knowledge is central. A program such as FCC aims to raise the rate of HIV testing by both improving HIV-related knowledge (raising perceived benefits of testing) and reducing HIV-related stigmatizing attitudes (reducing perceived costs of testing). Improvements in knowledge can raise HIV testing directly, as well as indirectly by reducing stigmatizing attitudes, which then also promotes testing.

There could, therefore, be substantial unintended consequences if the effort to improve knowledge related to HIV goes awry. Should a program inadvertently spread misinformation and worsen knowledge, it could reduce testing rates directly, as well as indirectly by worsening stigmatizing attitudes.

In our empirical work, we will explore this possibility by examining both HIV-related knowledge and stigmatizing attitudes as outcome variables, alongside HIV testing. As we have highlighted in the introduction, we found that the program has mixed impact on HIV-related knowledge (actually making certain knowledge areas worse), and increases HIV-related stigmatizing attitudes. This helps explain the very modest increases in HIV testing, much lower than expert forecasts.

At the same time, in our empirical work, we will highlight another impact of the FCC program that confirms this interpretation of the findings: an increase in safe sexual behaviors, and a reduction in the number of sexual partners. This is consistent with the increase we see in stigmatizing attitudes. As individuals fear HIV-related

stigma more, they may not only be less likely to get tested, they may also engage less in unsafe sex so as to reduce the likelihood of becoming infected.

## 2.3 Research Design

### 2.3.1 Country and Programmatic Context

Out of an estimated 36.9 million people living with HIV worldwide in 2017, 25.7 million are in Sub-Saharan Africa. The region also accounts for a dominant share of new HIV infections: 1.17 million out of a global 1.8 million in that year. In Mozambique in 2017, 2.1 million people out of a population of 29.7 million were living with HIV (7.1% of the population), out of which 170,000 were children (aged 14 or below). The country has an estimated 130,000 new HIV infections annually, of which 13.8% are children. Mozambique recorded 70,000 AIDS-related deaths in 2017, likely because only slightly more than half of HIV-infected patients have access to anti-retroviral therapy (ART). Poor access and adherence to ART contributes to AIDS-related morbidity and mortality, as well as HIV transmission (to other adults as well as from mothers to children) (UNAIDS, 2019).

The U.S. Government’s most important program responding to the HIV/AIDS crisis is the President’s Emergency Plan for AIDS Relief (PEPFAR), initiated in 2003. Recognizing that children are among the most vulnerable populations in the context of the HIV/AIDS pandemic, PEPFAR mandates part of its funding be devoted to programs benefiting children orphaned or made vulnerable by HIV/AIDS ( “orphans and vulnerable children,” or OVCs).<sup>12</sup> PEPFAR’s programs for OVCs take an integrated approach, with interventions at child, family, and community levels; that target child needs at different developmental stages; and that are connected to other development programs related to education, nutrition, and household economic de-

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<sup>1</sup>The UN defines an “orphan” as a child who has lost one or both parents. An estimated 13.4 million children and adolescents (0-17 years of age) worldwide had lost one or both parents to AIDS as of 2015. More than 80% of these children (10.9 million) live in sub-Saharan Africa (UNICEF, 2016).

<sup>2</sup>PEPFAR’s 2008 reauthorization mandated it to spend 10% of funds on assistance to OVCs. PEPFAR defines children as those below 18 years of age. These funds amounted to more than \$1 billion in 2006-09, and \$672 million in 2010-11. (PEPFAR Operational Plans for fiscal years 2006-2011, available at <http://www.pepfar.gov>.) In the 2015 fiscal year, PEPFAR spent \$218 million on OVC programming (PEPFAR, 2017).

velopment (PEPFAR, 2006). In fiscal year 2016, PEPFAR OVC programs supported 6.2 million OVCs and their caregivers worldwide (PEPFAR, 2017).<sup>3</sup>

### 2.3.2 The Intervention

The program we study, *Força à Comunidade e Crianças* (FCC, “Strengthening Communities and Children”), is a representative example of PEPFAR OVC programs. Its high-level aim is to improve families’ and communities’ ability to support, protect, and care for orphans and vulnerable children, their caregivers, and their households more generally.

While the FCC program is multifaceted and can affect many possible outcomes, this study focuses its primary analyses and hypotheses on central HIV testing outcome variables, the main program component, and a subset of mechanisms (intermediate outcomes) through which effects may operate. From this perspective, we will measure the program’s overall impacts, measure spillovers from program beneficiaries to other households, and provide evidence on mechanisms through which the program achieves its impacts. Other outcomes, program components, and mechanisms will be the subject of secondary analyses, which can provide guidance for the foci of future studies.

To be specific, our primary focus is on the following:

- **Outcome variable:** Having been tested for HIV in the past 12 months (self-reported); take-up of a recommendation to get a new HIV test (directly observed)
- **Program component:** Home visits by local implementing partner (LIP) staff (Case Care Workers, or CCWs)
- **Mechanisms / intermediate outcomes:** Information on HIV; information on ART; concerns about HIV-related stigma

HIV testing is the outcome variable of primary focus because it is the first, prerequisite step in the chain that then leads to the initiation of HIV treatment (ART) and ART adherence. The importance of HIV testing is strongly emphasized in the most central and widespread program components (OVC home visits by community

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<sup>3</sup>Reviews of research on OVCs include Bryant and Beard (2016), Goldberg and Short (2016), Nyberg et al. (2012), and Shann et al. (2013). See also Evans and Miguel (2007), Case, Paxson and Ableidinger (2004), Larson et al. (2013), and Whetten et al. (2014).



workers, and school-based programs) via information provision and efforts at reducing HIV-related stigmatizing attitudes. The more specialized and narrower program components (such as Village Saving and Loan Associations, or VSLAs, and youth groups) also systematically reinforce the importance of HIV testing.

We now describe the FCC program, highlighting in detail the outcome variables, program components, and mechanisms (intermediate outcomes) of primary interest. Other aspects of secondary interest will be described in less detail.

The FCC program is composed of a number of interrelated components, and is implemented in study districts by LIP organizations under contract to the international NGO World Education Inc./Bantwana. A number of FCC program components are school-based, and so programs are implemented in local communities surrounding a focal school. Some components are focused on children, others on adults. In each community, activities take place with the collaboration and advice of a Community Child Protection Committee (CCPC) whose membership includes community leaders, volunteers, and local government officials. The program is implemented in seven districts of three provinces of Mozambique.<sup>4</sup>

The most widespread FCC program component is **home visits** by LIP staff known as “Case Care Workers” (CCWs) to households in program communities. Roughly 700 CCWs work across the study communities. LIPs hire CCWs from the communities they serve, in part based on recommendations by the CCPC and community leaders. In common with the local populations they serve, they typically have no more than a primary school education. Roughly 80% of CCWs are female. They range in age from 18 to 48, with most falling between 25 and 40 years of age. CCWs receive a stipend of 3,100 MZN per month (roughly US\$150), as well as in-kind compensation in the form of a bicycle, a work uniform, and cellphone airtime.

CCWs conduct home visits of households thought likely to be OVC households, based on personal knowledge and recommendations of the CCPC. The home visit itself is a conduit for the dissemination of information and advice by CCWs, whose impacts we seek to measure. All household members may then participate in other FCC components, based on the results of the home visit. In home visits, CCWs conduct systematic vulnerability assessments, and identified “OVC” households (and individuals therein) are then linked to appropriate programs and services in commu-

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<sup>4</sup>Program provinces and districts are: Manica province (Manica, Chimoio, and Gondola districts), Sofala province (Dondo and Nhamatanda districts), and Zambezia province (Namacurra and Nioadala districts).

nities, schools, and health facilities. One of the most important results of these home visits is the referrals of individuals for HIV testing at the nearest PEPFAR-funded health clinic. The expectation is that CCWs refer all FCC program beneficiaries (both adults and children of all ages) who do not know their HIV status for HIV testing, and that even upon a negative test result testing should be repeated every twelve months. The number of individuals referred to HIV testing is a key outcome indicator for the FCC program, monitored by PEPFAR in the context of achieving the UNAIDS 90-90-90 global goals (90% of those with HIV diagnosed, 90% of those on ART, and 90% of those virally suppressed by 2020 (PEPFAR, 2017) ). Those testing positive for HIV are then referred to receive ART through the clinic. CCWs in the community then follow up with individuals initiating ART to promote ART adherence on an ongoing basis. Because of the centrality of encouraging HIV testing in the FCC program, it is the primary outcome of interest in this study.

During initial and subsequent home visits, CCWs undertake activities to increase HIV testing rates via two mechanisms we will examine explicitly: improving information and reducing stigma concerns. CCWs seek to improve FCC beneficiaries' **information related to HIV/AIDS**, such as methods of disease transmission, progression of the disease, treatment, HIV testing, and locations of health clinics providing testing and treatment. Information is conveyed verbally and, at the LIP's discretion, on printed material given to the household. In addition, CCWs are expected to engage program beneficiaries in "sensitization" to address **stigma related to HIV** (both one's own stigmatizing attitudes, and fear of stigma from others). CCWs engage in discussions to reduce stigmatizing attitudes among program beneficiaries. CCWs provide psychosocial support (PSS) and gradually gain program beneficiaries' trust over time in repeated interactions, with the expectation that reductions in fear of stigma will encourage people to be open to HIV testing, voluntarily disclose HIV-positive status to CCWs, and be open to future CCW follow-up promoting ART initiation and adherence.

In home visits, CCWs are also expected to give caregivers advice and encouragement regarding **children's education**. Caregivers are encouraged to make sure children go to school daily, have appropriate materials and uniforms, and have a place to study at home without distractions. They are encouraged to be involved in their children's education, such as by establishing contact with a child's teachers, maintaining contacts with a child's friends, and helping with homework. Caregivers are also encouraged to discourage girls' early marriage, and to keep girls in school even

after the age of 18. Given the prominence of education advice and encouragement in the home visit, child school attendance is a secondary outcome variable in the study.

The FCC program has a number of other components. Households are connected to these other components after the home visits, based on needs assessments conducted by CCWs. Many components are school-based, so children can also be included in these components through their schools. In practice, very small shares of households have participated in these other program components, so we believe they are likely to contribute only a small part of the treatment effects we highlight in this paper.

### 2.3.3 Sample

The unit of analysis will be the household or individual, depending on the outcome variable. Hypotheses related to HIV testing focus on household-level outcomes because the intervention components related to HIV testing are delivered at the household level (not at the individual level). In addition, there is a correlation of attitudes and testing within the household, so we can reasonably think of “household-level” decision-making regarding HIV testing.

The sample was constructed as follows. We administered a baseline survey in 2017-18 to households in the 76 communities using random-route sampling, with starting points at the focal school in each community. We then sought to survey these “main list” households again in the 2019 endline survey. Given our staff and timing constraints, we could only offer households the Randomization Stage 3 treatments (described below) and HIV testing coupons if we were able to locate and survey them in roughly the “regular round” of the endline survey in each community (roughly the first week of surveying in each community). We were able to reach 80% of main list households in this manner. To raise our sample size, we then supplemented this set of households with a “back-up list” of households who were administered a short screening survey in the baseline phase, until we reached a target of 60 households in each community. These households from the back-up list were also offered HIV testing coupons, and also enter our sample for analysis.

We later implemented an “intensive follow-up round” of the endline survey to intensively track and survey the 20% of “main list” households who we were unable to initially survey in the regular round. These households were found later, after we had completed the Randomization Stage 3 treatments and the HIV testing coupon redemption and tracking. Therefore, these households do not have the HIV testing

outcome based on coupon redemption, and never received the Randomization Stage 3 treatments. These households are not included in the analysis sample in the current paper.<sup>5</sup> These households will be included in future papers that involve longer-term follow-up of these households in subsequent survey rounds.

The sample is composed of 4,240 households, composed of 16,925 individuals, of whom 11,751 are children (aged below 18) at the time of the endline survey.

### 2.3.4 Methodology: Random Assignment

This study aims to provide convincing estimates of causal and spillover effects of the FCC program using a randomized controlled trial (RCT) methodology. Random assignment allows estimated relationships to be interpreted as causal effects, rather than simply correlations.

Our approach involves a three-stage randomized controlled trial methodology to estimate causal direct and spillover effects of the FCC program, and to shed light on some of the operative mechanisms through which it achieves its effects.

First, communities were randomly assigned to treatment or control status (inclusion in or exclusion from the FCC program). Second, a subset of households within treatment communities were randomly assigned to a strong encouragement to participate in FCC programs ( “directly enrolled” households). These directly enrolled households receive a home visit by an FCC program community worker and are assessed for inclusion in various FCC subcomponent programs. This will lead them to have higher participation rates in the program than other households in treatment communities. Other households not randomly selected for direct enrollment end up being treated as well, but at lower rates. These first two randomization components were carried out in 2017, and led to varying household exposure to the FCC program throughout 2018. They allow us to shed light on direct impacts, and to quantify spillovers.

The third part of the randomized methodology is aimed at shedding light on the extent to which the FCC program complements or substitutes for HIV information, efforts to combat HIV-related stigma, and financial incentives for HIV testing. We are interested in complementarity with information interventions focused on improving *information about HIV*, improving *information about HIV treatment (ART)*, and re-

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<sup>5</sup>All other results we report in this paper (other than HIV testing based on coupon use, or those related to the Randomization Stage 3 treatments) hold in an expanded sample that includes households found in the second round of the endline survey.

ducing *concerns about HIV-related stigma*. We are also interested in complementarity with *financial incentives for HIV testing*. We randomly assign simple treatments at the household level that our project staff administer immediately after the endline survey. (These treatments are detailed below.) Complementarities could be positive or negative (the FCC program could magnify or reduce the impact of such later more targeted interventions). If these treatments are found to have smaller effects on directly-enrolled households in treatment communities than on households in control communities, this would be evidence that the FCC program and these more targeted interventions are substitutes. It is also possible that these treatments could be complementary with the FCC program: they could have larger effects on directly-enrolled households in treatment communities than on households in control communities.

### Randomization Stage 1

The FCC program is a community-level intervention, so the first stage was random selection of communities to receive or not receive the FCC program. FCC interventions are centered in primary and secondary schools, so geographic areas of interest are residential areas surrounding schools. (We refer to areas surrounding schools simply as “communities”, each of which has a “focal school” where school-based program components are implemented.) World Education Inc./Bantwana consulted with local implementing partners (LIPs) and government officials in the three provinces and seven districts in which the FCC program was to be implemented to identify a set of 76 communities deemed to be “eligible” for the program. These communities were chosen on the basis of being geographically proximate to ART sites (health clinics offering HIV testing and treatment), having sufficient populations of orphans and vulnerable children (OVCs), and having no other active donor-funded HIV/AIDS programs. These 76 communities were then sorted into stratification cells of matched community pairs, sets of two communities that were very similar in terms of distance to ART sites, school type (secondary or primary), and student population size.

Within each matched pair, treatment status was randomly assigned to one community, with the other school assigned to control status. Randomization of treatment status within matched pairs helps ensure balance in baseline characteristics between treatment and control units, so that treatment-control comparisons can then be credibly interpreted as causal effects of the program. This random assignment was carried out on the computer of one of the co-authors, one-time, with no re-randomization.

The result of the randomization was communicated to World Education/Bantwana in November 2016. The FCC program was then implemented in treatment communi-

ties, and not in control communities. School-based components of the program were implemented in the focal school in each treatment community, and not in control communities.

### Randomization Stage 2

The second stage of randomization, at the household level, was implemented only within treatment communities.

Of households originally contacted and consented by the study team, a subset were randomly assigned to be “directly enrolled beneficiaries” (DEBs) of the FCC program: their geographic coordinates and household head’s name and contact information were provided to World Education/Bantwana and their local implementing partners (LIPs). LIP staff (CCWs) then conducted household and individual assessments for FCC program subcomponents. Analyses facilitated by this random assignment to DEB status are outlined below.

Random assignment of households to direct FCC enrollment was carried out in November and December 2017 on the computer of one of the co-authors, one time, with no re-randomization. Out of the 40 OVC households administered the baseline survey in each treatment community, 15 were randomly assigned to DEB status (so 25 baseline households in each treatment community have non-DEB status). In addition, to enhance statistical power, we also randomly assigned DEB status to 20 households who received a shorter Vulnerability Assessment (VA) survey but not the full baseline survey in each treatment community.<sup>6</sup> Therefore, a total of 35 households in each treatment community received DEB status.

This stage of randomization had two motivations. First, it creates a subgroup of households in treatment communities with relatively high take-up or participation in the FCC program. Estimates of the impact of the FCC program comparing this group to households in control communities, therefore, have higher statistical power. We pursued this approach because of a fear of low statistical power for treatment effect estimates based on generally comparing households in treatment and control communities. The second motivation is to measure spillovers of impacts to other households. Because DEBs were randomly selected, non-DEB households have random geographic and social proximity to DEB households. This facilitates credible measurement of spillovers from DEB to non-DEB households.

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<sup>6</sup>This latter group of 20 households were also on the main list for surveying in the regular round in treatment communities, as were a randomly-selected group of 20 (VA-only, no-baseline) households in control communities for comparison purposes.

### *Randomization Stage 3*

To understand complementarities between the FCC program and more targeted interventions, our research team provided additional treatments after the administration of the endline survey: HIV/AIDS information, HIV treatment (ART) information, information to reduce stigma concerns, and higher financial incentives for HIV testing.

Households participating in the regular round of the endline survey were randomly assigned to one of the six groups described below, with equal probability. The treatments were only administered to consenting survey respondents.

- 1) **Anti-stigma:** individual-specific information aimed at reducing the respondent's concerns about HIV-related stigma in the community.
- 2) **HIV/AIDS Information:** factual information about HIV/AIDS.
- 3) **Antiretroviral Therapy (ART) Information:** factual information about ART.
- 4) **Both HIV/AIDS and ART Information:** the combination of items 2 and 3 above.
- 5) **High incentive for HIV testing:** each HIV testing coupon offered to the household provides a financial incentive of 100 MZN (instead of 50 MZN for everyone else).<sup>7</sup>
- 6) **Control:** None of the above treatments.

These treatments are randomly assigned on the computer of one of the co-authors one time, with no re-randomization. The randomization is stratified by the community, DEB status, and baseline asset level. The Randomization Stage 3 treatments are independent of (orthogonal to) the Stage 1 and Stage 2 randomizations. The sample sizes for each of the treatments are shown in Table 2.1.

Table 2.1 presents the full cross-cutting set of treatments, indicating the number of households per cell.

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<sup>7</sup>The PPP conversion factor of the metical (MZN) is 20.62 in 2018. 50 MZN converts to 2.42 USD and 100 MZN to 4.85 USD. As a comparison, the poverty headcount ratios at \$1.90 (2011 dollar) a day and \$3.20 (2011 dollar) a day are 62.4% and 81.5% of the country's population, respectively. The poverty population in Mozambique roughly matches the sample from OVC households as defined in this study, which takes up 71.6% of the general population in the study region. A coupon of 50 MZN can approximately cover the daily cost of living of a study participant. Data source: The World Bank Data (<https://data.worldbank.org>).

Table 2.1: Treatment Assignment and Sample Size

Randomization Stage 3 Groups	FCC Treatment		FCC Control	Total
	DEB	Non-DEB		
Anti-Stigma	192	201	377	770
HIV/AIDS Information	177	210	375	762
ART Information	209	192	381	782
HIV/AIDS, ART Info. Combined	145	145	286	576
High Testing Incentive	145	138	283	566
Control	200	200	384	784
Total	1,068	1,086	2,086	4,240

## 2.4 Hypotheses

We detailed our empirical analyses in a pre-analysis plan submitted to the American Economic Association’s RCT Registry (registration ID number AEARCTR-0003990) on March 8, 2019, which was prior to the start of our endline survey fieldwork.

### 2.4.1 Primary hypotheses

The primary question of interest in this study is: what are the direct effects of the program on HIV testing in FCC beneficiary households?

We address this question by estimating the causal effect of a household being a directly-enrolled beneficiary (DEB) of the FCC program, all of whom are in treatment communities. In estimating this effect, all households in control communities will be the control group. (Non-DEB households in treatment communities are the subject of secondary analyses.)

Our primary analyses test whether household assignment to strong encouragement for participation in the FCC program (which we refer to as directly-enrolled beneficiary or “DEB” status) leads to higher rates of HIV testing in the household. HIV testing is the outcome variable of primary focus because it is a prerequisite for benefiting from the FCC program in the health domain.<sup>8</sup> HIV testing opens the door to FCC interventions promoting ART treatment initiation and adherence. In addition,

<sup>8</sup>Our primary outcome variables measure HIV testing for both adults and children. The health of adults (in particular, their HIV status) is an important determinant of the outcomes of children in their households; HIV testing can lead adults to learn they are HIV positive, leading them to initiate ART, with positive effects on children in their households. When it comes to children (those aged below 18), HIV testing is important as well, most importantly, after puberty and sexual debut



HIV testing is emphasized and encouraged in the context of major FCC program components (OVC home visits by community workers, and school-based programs). The more specialized and narrower program components (such as VSLAs and youth groups) also systematically reinforce the importance of HIV testing.

The outcome variable of primary interest is HIV testing at the household level. This will be a binary outcome indicating that the household either self-reports having had or is directly observed by our survey staff having an HIV test upon our recommendation. This outcome captures the combination of having already had an HIV test, as well as responsiveness to recommendations for future testing, both of which may be influenced by exposure to the FCC program.

To be specific, the component variables of this outcome variable are:

- HIV testing (self-reported): an indicator equal to 1 if at least one household member is reported to have had an HIV test in the last 12 months, and 0 otherwise.
- HIV testing (directly observed): an indicator that at least one of a household's HIV testing coupons has been redeemed. This is a household-level variable equal to 1 if at least one of a household's incentive coupons is presented at the local health clinic for the HIV testing incentive payment before the 14-day deadline, and 0 otherwise.<sup>9</sup>

Our composite HIV testing outcome is therefore equal to 1 if HIV testing (self-reported) is equal to 1 or HIV testing (directly observed) is equal to 1, and 0 otherwise.

*Primary Hypothesis: Assignment of a household to DEB status raises rates of HIV testing in households, compared to households in control communities.*

## 2.4.2 Secondary Hypotheses

A number of secondary hypotheses are of interest, related to impacts on other outcomes, mechanisms of impacts on DEBs, impacts on non-DEB households, and spillovers from DEB to non-DEB households.

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leads to non-trivial rates of new HIV infection. There are also much lower but nonzero rates of HIV infection from mothers (or other household members) to younger children.

<sup>9</sup>The directly observed variable is coded as zero for households refusing any incentive coupons, which is rare. Another rare case is households with no-one eligible for coupons because of everyone having been tested within the last three months; in this case, the directly observed variable will again be set to zero.

First, we examine the two HIV testing variables separately, without combining them into one composite outcome. These two outcomes are worth examining separately, because they measure distinct things, and each has strengths and weaknesses. HIV testing in the last 12 months is of greater research and policy interest, because it is not financially incentivized and therefore is the “natural” context in which the HIV testing decision is made. But this outcome measure has the downside that it is self-reported, and may be subject to reporting biases; in particular, it is likely to be overstated by households in the survey, particularly in treatment locations and for DEB households. We therefore complement this measure with a directly-observed measure: the redemption of the coupons incentivizing HIV testing. Because the take-up of the coupons is directly observed, it has an important strength: it is immune from survey-reporting biases. The downside of this measure is that the HIV tests are financially incentivized, which departs from the general context of HIV testing. We believe the financial incentive is necessary to ensure the respondents turn in the coupons to our research staff at clinics (without submission of the coupons, there would be no way to measure take-up of testing).

We pre-specified secondary outcomes and hypotheses related to the impact of DEB status on other outcome variables. The secondary hypotheses are that assignment of a household to DEB status:

- raises rates of school attendance among children in the household.
- raises life satisfaction, household assets, and ART adherence rates.
- raises HIV-related knowledge, reduces HIV-related stigmatizing attitudes, increases other positive attitudes towards HIV, and reduces rates of risky sexual behavior.

We also pre-specified secondary outcomes and hypotheses related to the impact of non-DEB status on the same set of outcome variables for the DEB hypotheses. These hypotheses are that households assigned to non-DEB status will have:

- higher rates of HIV testing
- higher rates of HIV testing, as measured by separately by the self-reported and directly observed outcome variables
- higher rates of school attendance among children
- higher life satisfaction, household asset indices, and ART adherence rates

- HIV-related knowledge, lower HIV-related stigmatizing attitudes, higher rates of other positive HIV-related attitudes, and lower rates of risky sexual behavior

We also have secondary hypotheses related to spillovers from DEB to non-DEB households in treatment communities. Given that not all households in a community directly benefit from the program, to what extent do impacts spill over from directly-affected households to others that are geographically or socially proximate? One key channel through which spillovers may occur is information: DEBs may share information with proximate non-DEBs. In addition, stigma may be a key mechanism, if reduced stigma by DEBs leads non-DEBs in proximity to them to be more willing to take up HIV testing. Other channels are possible.<sup>10</sup> The outcome of interest for this analysis is the composite measure of HIV testing. Right-hand-side variables of interest are measures of social and geographic proximity to DEBs. We define and discuss these proximity measures when we discuss the spillover analyses below.

The secondary hypotheses also include those related to the Randomization Stage 3 treatments. For these treatments, the outcome of interest is the directly-observed measure of HIV testing (incentive coupon redemption) at the household level, as described above. (This is the only outcome measure that is observed after the endline survey.)

We will estimate the causal impacts of the Randomization Stage 3 treatments on HIV testing, and the extent to which their effects vary according to a household's treatment status (DEB, non-DEB, and control). If these treatments have less impact on HIV testing for treated than in control households, we will take this as evidence the FCC program and these more targeted treatments are substitutes. Complementarity, on the other hand, would be revealed if these targeted treatments have larger impact for treated than control households. We also examine whether the effects of the Randomization Stage 3 treatments on HIV testing differ for non-DEB households in treatment communities, compared to households in control communities.

## 2.5 Empirical Analyses

We test hypotheses using ordinary-least-squares regression analyses. We cluster standard errors at the level of 76 communities (Moulton, 1986).

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<sup>10</sup>These other channels include health channels, such as via contagion, or financial channels, if DEBs benefit financially from the program and transfer resources to non-DEBs.

To estimate the impact of directly-enrolled beneficiary (DEB) and non-DEB status, the regression equation will be as follows:

$$Y_{ijs} = \alpha + \beta B_{ijs} + \lambda N_{ijs} + \gamma_s + \epsilon_{ijs}. \quad (2.1)$$

$Y_{ijs}$  is the post-treatment outcome for individual or household  $i$  in community  $j$  in stratification cell (matched pair)  $s$ .  $B_{ijs}$  is the indicator for a household being randomly assigned to directly-enrolled beneficiary (DEB) status (1 if DEB, and 0 if not), while  $N_{ijs}$  is the indicator for a household being randomly assigned to non-directly-enrolled beneficiary (non-DEB) status in a treatment community (1 if non-DEB, and 0 if not). (Both variables are equal to zero for anyone in a control community. In other words,  $B_{ijs}$  and  $N_{ijs}$  simply partition households in treatment communities into two mutually exclusive subgroups.)  $\gamma_s$  is a fixed effect for stratification cell  $s$ .<sup>11</sup>  $\epsilon_{ijs}$  is a mean-zero error term.

The coefficient  $\beta$  is the intent to treat (ITT) effect of assignment to DEB status (high probability of home visit by a CCW), while the coefficient  $\lambda$  is the corresponding effect of assignment to non-DEB status (receiving a CCW home visit at the lower ambient rate in the community). Random assignment of DEB status allows interpretations of these coefficients as causal effects.

This regression will be used to test hypotheses related to the impact of random assignment to DEB status and non-DEB status within treatment communities. Hypothesis tests regarding the impact of DEB status will refer to coefficient  $\beta$  in this regression for the relevant outcome variable. Hypothesis tests regarding the impact of non-DEB status will refer to coefficient  $\lambda$  in this regression for the relevant outcome variable.

## 2.5.1 Effects of DEB and Non-DEB Status

### 2.5.1.1 Balance and Attrition

It is important to confirm the balance of baseline variables with respect to treatment assignment. We examine eleven variables that were collected in the baseline survey round (in 2017-18). These are dependent variables in estimation of

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<sup>11</sup>The inclusion of the stratification cell fixed effects reduces standard errors by absorbing residual variation. Stratification is at the level of 38 matched pairs of communities within which treatment status was randomly assigned (so stratification cell fixed effects are equivalent to matched pair fixed effects).

Equation (2.1). We report the results in Table 2.2. None of the coefficients on the DEB coefficient are large or statistically significant at conventional levels. Among the non-DEB coefficients, only one is statistically significant at conventional levels (the one in the regression for having a ratio of children to adults greater than four). This share of significant coefficients is about what one would expect to see by chance. These results provide no indication of a substantial imbalance in baseline household characteristics across treatment conditions.

Table 2.2: Balance of Baseline Household Characteristics

Vulnerability Indicators	Control Group Mean	Coef. on DEB	Coef. on Non-DEB	Coef. on Treatment	p-value of test: DEB = nonDEB
A grandparent of children is the household head	.304	-.00187 (.0156)	-.0205 (.0149)	-.0113 (.0121)	.313
Ratio of children to adults $\geq 4$	.0709	.0180 (.0134)	.0309** (.0138)	.0245* (.0123)	.265
There are school-aged children that are not in school	.305	.0227 (.0189)	.0171 (.0177)	.0199 (.0143)	.807
Household eat less than 2 meals a day	.0134	-.00125 (.00362)	3.74e-5 (.00369)	-.0006 (.00308)	.745
Household go some days without food	.597	.0253 (.0277)	.0193 (.0230)	.0223 (.0230)	.785
Primary income source illegal or no source of income	.0221	-.00706 (.00596)	.00325 (.00552)	-.00186 (.00520)	.0430
There are chronically ill household members	.226	-.00452 (.0195)	.00524 (.0190)	.000396 (.0177)	.524
There are HIV positive household members	.157	-.00734 (.0147)	-.0100 (.0159)	-.00869 (.0141)	.824
There are household members on ART medications	.118	.0112 (.0135)	.00710 (.0131)	.00915 (.0119)	.732
There are orphaned children	.270	.0116 (.0197)	.0317 (.0211)	.0217 (.0183)	.271
There were adults died of chronic illness in the last 5 years	.0911	.000996 (.0105)	.00823 (.00988)	.00464 (.00884)	.479

**Notes:**Standard errors in parentheses. Standard errors are clustered at the community level. All regressions control for matched-pair fixed effects. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Number of observations = 4,240.

Another key question is whether success in locating households from the main list in the regular round of the endline survey (which determines inclusion in the analysis sample) is affected by treatment status. If so, this raises concerns about selection bias due to differential attrition. We examine this by regressing an indicator variable

for being surveyed in the regular round on treatment indicators and stratification cell (matched pair) fixed effects. The results are in Table 2.3. The mean of the dependent variable in the control communities is 0.799. The coefficient on the indicator for being a directly-enrolled beneficiary (DEB) of the FCC program is very small in magnitude and not statistically significantly different from zero at conventional levels. The coefficient on non-DEB status is positive and modest in size (0.033), and statistically significant, indicating that non-DEB households in treatment communities are slightly more likely to have been successfully surveyed in the regular round of the endline survey.

Table 2.3: Main list Attrition Analysis

Group Indicators	Surveyed in the regular follow-up
DEB	-0.00500 (0.0121)
Non-DEB	0.0329** (0.0138)
Observations	4,546
R-squared	0.061
Control Group Mean	0.799
p-value of test DEB = nonDEB	0.0170

**Notes:** Standard errors in parentheses. Standard errors are clustered at the community level. All regressions control for matched-pair fixed effects. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

These results indicate no concern with selection bias for our pre-specified primary coefficient of interest, on DEB status, since DEB status is not associated with attrition. They do raise the possibility of selection bias due to differentially lower attrition related to non-DEB status. This should be kept in mind when interpreting coefficients on non-DEB status.<sup>12</sup> (Note that we pre-specified that the treatment effect of non-DEB status is only of secondary interest in the analysis.)

### 2.5.1.2 “First Stage” Impacts on Contacts with FCC Program

As a starting point for understanding any treatment effects to come, it is useful to examine impacts on outcomes measuring knowledge of, contact with, and services pro-

<sup>12</sup>That said, we do not find evidence of major worries related to the selectivity of the non-DEB households. Controlling for a full set of baseline variables does not have an appreciable effect on the non-DEB coefficients in our analyses, providing no evidence of concerns about selection bias in the non-DEB coefficient estimates.

vided by the FCC local implementing partner (LIP) organization. While we are not conducting instrumental variables (IV) estimation, these could be considered “first stage” outcomes, confirm and measure the extent to which the FCC program reached the intended beneficiaries. These outcomes come from the endline survey, reported by the primary household respondent. We examine an indicator for a household having heard of the LIP, an indicator for a household having been visited by a Case Care Worker (CCW) of the LIP, and an indicator for a household having been referred to or received any services from the LIP. The indicator is constructed from several survey questions asking about services received from non-government organizations (NGOs), and which organization provided these services.

Regression results from estimation of Equation (2.1) for these first stage outcomes are in Table 2.4. Being a DEB leads to higher rates of having heard of, been contacted by, or received services referred by the LIP. Non-DEB status also has positive effects on these outcomes, indicating that LIPs also reached households in the community in general. All coefficients on DEB and non-DEB status are all statistically significant at the 1% level.

DEBs did have higher rates of contact with the FCC program than non-DEBs. For each outcome, coefficients on DEB status larger in magnitude than the corresponding coefficient for non-DEBs. For the “contacted by” and “referred service” regressions, the difference between the DEB and non-DEB coefficients is statistically significantly different from zero at conventional levels (p-values 0.082 and 0.006, respectively, reported in the bottom row of the table.)

These results indicate that the FCC program did differentially reach households in treatment communities than in control communities, and DEBs more than non-DEBs in treatment communities.<sup>13</sup> That said, the contact and referral rates for DEBs are lower than we expected in advance. WEI/Bantwana reports (based on data collected from LIPs) that 77.0% of households assigned to DEB status were successfully administered a home visit by a CCW.

By contrast, our estimates imply that only 12.3% of DEBs were contacted by LIPs, and only 19.2% were referred to any service by LIPs. These findings potentially shed a negative light on the accuracy of WEI/Bantwana’s reports about Randomization Stage 2 implementation. It is also possible that households are underreporting the

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<sup>13</sup>Note each of the outcome variables have means that are nonzero in control communities. This is to be expected, because LIPs tend to be well-established organizations and have other activities separate from those they are contracted to undertake as part of the FCC program.

Table 2.4: “First Stage” Impacts on Contacts with FCC Program

Group Indicators	(1) Heard of LIP	(2) Contacted by LIP	(3) Referred service by LIP
<b>Panel A</b>			
DEB	0.132*** (0.0254)	0.0676*** (0.0115)	0.102*** (0.0197)
Non-DEB	0.108*** (0.0253)	0.0430*** (0.0112)	0.0709*** (0.0179)
R-squared	0.120	0.068	0.089
<b>Panel B</b>			
Treatment	0.120*** (0.0233)	0.0552*** (0.00896)	0.0863*** (0.0180)
R-squared	0.120	0.067	0.088
Observations	4,240	4,240	4,240
Control Group Mean	0.488	0.0551	0.0916
p-value of test DEB = nonDEB	0.231	0.0820	0.00600

**Notes:** Standard errors in parentheses. Standard errors are clustered at the community level. All regressions control for matched-pair fixed effects. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1



extent to which they had interactions with LIPs, perhaps because LIPs interacted with a different household member than the survey respondent, the survey respondent has forgotten the interaction with the LIPs, or the survey respondent did not correctly report that the identity of the organization with which the household had a contact or referral.

Whatever the reason for the lower-than-expected rates of interaction with the FCC program, these low rates may be one reason why treatment effects on other outcomes reported below are relatively modest in size. On the other hand, any treatment effects that we do find should be interpreted as lower bounds on the effect of an FCC-like program if it were to be implemented with higher rates of penetration into the population.

### **2.5.1.3 Primary Analysis**

We now turn to tests of the primary hypotheses of the paper: impacts on HIV testing. Results are presented in Table 2.5.

The coefficient on the pre-specified primary outcome of interest, the composite HIV testing measure (Column 1) is positive, but modest in size and not statistically significantly different from zero at conventional levels. The point estimate indicates a 2.2 percentage point increase in testing rates, relative to the 72.3% rate in control communities.

Coefficients on the pre-specified secondary outcomes, the HIV testing measures considered separately, are also small in magnitude and not statistically significantly different from zero at conventional levels. The point estimate on the HIV test self-report (Column 2) is positive and similar in magnitude to the coefficient in Column (1). The point estimate on the HIV test based on coupon use (Column 3) is actually negative in sign.

By contrast, the coefficients on the non-DEB indicator (pre-specified as of secondary interest) in the three columns are all positive in magnitude, in magnitudes indicating 2.4-3.9 percentage point increases in testing rates. The non-DEB coefficients are actually statistically significant at the 10% level in Column (1)-(2). In Column (3), the non-DEB coefficient is statistically significantly different from the coefficient on the DEB indicator (p-value 0.010, reported in the bottom row of the table).

Table 2.5: FCC Impacts on HIV Testing

Group Indicators	(1) HIV test	(2) HIV test: self-report	(3) HIV test: coupon use
<b>Panel A</b>			
DEB	0.0221 (0.0196)	0.0245 (0.0242)	-0.0218 (0.0179)
Non-DEB	0.0350* (0.0185)	0.0386* (0.0225)	0.0243 (0.0183)
R-squared	0.033	0.033	0.057
<b>Panel B</b>			
Treatment	0.0286* (0.0169)	0.0316 (0.0216)	0.00139 (0.0158)
R-squared	0.033	0.033	0.056
Observations	4,179	4,179	4,240
Control Group Mean	0.723	0.626	0.264
p-value of test DEB = nonDEB	0.466	0.434	0.0100

**Notes:** Standard errors in parentheses. Standard errors are clustered at the community level. All regressions control for matched-pair fixed effects. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

It is additionally informative to compare our treatment effect estimate to expert predictions elicited in advance. Prior to our results being known, in DellaVigna, Otis and Vivalt (2020) collected from subject-matter experts their forecasts of the treatment effect of being assigned to DEB status in a treatment community on the 12-month self-report of HIV testing.<sup>14</sup> The mean expert prediction was 11.36 percentage points. Our actual treatment effect, 2.45 percentage points (Column 2), is substantially below the expert prediction: it is only 21.6% as large in magnitude, and a Wald test rejects the hypothesis of equality of two at conventional levels (p-value 0.0004).

In our pre-analysis plan, we stated that if results on HIV testing differed between the self-reported (Column 2) and directly-observed (Column 3) measures of HIV testing, we will base substantive conclusions and policy recommendations on the findings that use the directly-observed outcome. Prioritizing the result in Column (3) provides an even more pessimistic assessment of the performance of the FCC program in promoting HIV testing.

The modest size of the effect of DEB status, and the fact that non-DEB status may if anything actually have more positive effects on HIV testing, are a first indication that the FCC program appears to be having unintended consequences. To explore what these unintended consequences might be, we now turn to additional empirical estimates, which will be a combination of pre-specified secondary analyses as well as exploratory (not pre-specified) analyses.

#### **2.5.1.4 Secondary Analyses**

##### *Potential mechanisms behind HIV testing results*

In Table 2.6 we examine impacts on hypothesized mechanisms behind the impacts on HIV testing: information, stigmatizing attitudes, and safe sexual behavior. DEB status has no large impact on the index of HIV knowledge, other positive HIV attitudes, or the safe sex behavior index. By contrast, the coefficient on the DEB indicator is negative in the regression for the HIV stigma attitudes index and for the number of sex partners in the last 12 months.

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<sup>14</sup>DellaVigna, Otis and Vivalt (2020) elicited predictions from 73 experts, mostly in December 2019. The online survey eliciting predictions closed on January 3, 2020. This process was completely arms-length from us. We proposed five names of potential expert forecasters to DellaVigna et al, but had no knowledge of the identities of the ultimate set of expert forecasters.

Table 2.6: FCC Impacts on Knowledge, Stigma, and Sexual Behavior

Group Indicators	(1) HIV knowledge index	(2) HIV stig- matizing attitudes index	(3) HIV other positive attitudes index	(4) Safe sex behavior index	(5) # of sex partners last 12 months
<b>Panel A</b>					
DEB	-0.00562 (0.00842)	-0.0123** (0.00519)	-0.00700 (0.0168)	0.00521 (0.00604)	-0.0969*** (0.0308)
Non-DEB	-0.00359 (0.00876)	-0.00634 (0.00590)	-0.0126 (0.0164)	0.00334 (0.00605)	-0.0984*** (0.0335)
R-squared	0.062	0.026	0.066	0.080	0.011
<b>Panel B</b>					
Treatment	-0.00459 (0.00753)	-0.00924* (0.00488)	-0.00986 (0.0148)	0.00427 (0.00529)	-0.0977*** (0.0283)
R-squared	0.062	0.026	0.066	0.080	0.011
Observations	4,584	4,450	4,536	4,577	4,517
Control Group	0.756	0.744	0.537	0.669	1.115
Mean					
p-value of test DEB = nonDEB	0.807	0.274	0.713	0.750	0.961

**Notes:** Standard errors in parentheses. Standard errors are clustered at the community level. All regressions control for matched-pair fixed effects. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Given these results, in exploratory analyses, we examine subcomponents of the HIV knowledge and stigma indices to get a more detailed sense of what may be driving these findings. We first examine the HIV information questions in greater detail. We divide the 33 questions into thematic subgroups and create sub-indices in a way analogous to the overall knowledge index. We report the regression results for treatment impacts on these sub-indices in Table 2.7. Underlying the lack of impact of the DEB status on the overall knowledge index, the impact on sub-indices is positive in some cases and negative in others. This can be described as a mixed set of impacts on knowledge. What is striking, however, is the negative impact on the index of “transmission myths”. These are questions about whether HIV can be transmitted in certain ways, all of which are not transmission channels (in other words, correct answers to these questions are always “no”): mosquito bites, shaking hands, kissing, sharing food, or witchcraft. This impact on the transmission myths index is negative (meaning an increase in false beliefs that these are in fact transmission channels), and is statistically significant at the 10% level. In regressions that are not shown (but available on request), we have run regressions on the individual questions comprising the index. We find that DEB status leads to higher rates of incorrect beliefs about each of these false transmission channels, and that the impact is statistically significantly different from zero for shaking hands, sharing food, and witchcraft.

We also examine the four separate components of the HIV stigma index as outcome variables, to see what responses are driving the change in stigmatizing attitudes. We report the regression results in Table 2.8. While none of the coefficients on DEB are statistically significantly different from zero on their own, the coefficient on DEB in the regression for “would not keep it a secret if a family member had HIV” is negative and the largest in magnitude in the table, and is statistically significantly different from the (slightly) positive coefficient on non-DEB status in the same regression. This is suggestive evidence, indicating that the increase in stigmatizing attitudes associated with DEB status is driven by increased reported desires to keep a family member’s HIV-positive status secret.

In the context of our conceptual framework, we view these results as revealing reasons why the FCC program had such modest impacts on testing: worsened knowledge about transmission methods could have increased stigmatizing attitudes, and the increase in stigmatizing attitudes had a negative impact on testing rates.

#### Other secondary outcomes

Table 2.7: FCC Impacts on HIV-related Knowledge

Group Indicators	(1) General HIV knowledge	(2) Correct ways of transmis- sion	(3) Transmission myth	(4) Protection methods	(5) Knowledge about the treatment for HIV
<b>Panel A</b>					
DEB	-0.00401 (0.0100)	-0.00787 (0.0134)	-0.0292* (0.0148)	0.00170 (0.00908)	0.00182 (0.00997)
Non-DEB	-0.00176 (0.00940)	-0.0137 (0.0126)	-0.0311* (0.0162)	-0.00147 (0.00878)	0.0142 (0.00964)
R-squared	0.051	0.038	0.068	0.054	0.065
<b>Panel B</b>					
Treatment	-0.00287 (0.00896)	-0.0108 (0.0112)	-0.0302** (0.0139)	8.77e-05 (0.00743)	0.00813 (0.00883)
R-squared	0.051	0.038	0.068	0.054	0.064
Observations	4,584	4,584	4,584	4,584	4,584
Control Group	0.621	0.831	0.749	0.821	0.772
Mean					
p-value of test DEB = nonDEB	0.765	0.655	0.893	0.750	0.143

**Notes:** Standard errors in parentheses. Standard errors are clustered at the community level. All regressions control for matched-pair fixed effects. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table 2.8: FCC Impacts on HIV-Related Stigmatizing Attitudes

Group Indicators	(1) Would buy fresh food from PLHIV	(2) Would NOT keep secret if family member HIV+	(3) Would take care of family sick with AIDS	(4) Would allow HIV+ teachers at school
<b>Panel A</b>				
DEB	-0.0136 (0.0101)	-0.0233 (0.0192)	-0.00424 (0.00311)	-0.00365 (0.00636)
Non-DEB	-0.0227** (0.0113)	0.00945 (0.0183)	-0.00217 (0.00268)	0.000589 (0.00513)
R-squared	0.040	0.048	0.015	0.027
<b>Panel B</b>				
Treatment	-0.0182** (0.00834)	-0.00662 (0.0174)	-0.00318 (0.00201)	-0.00149 (0.00454)
R-squared	0.040	0.047	0.015	0.027
Observations	4,374	4,397	4,425	4,358
Control Group Mean	0.857	0.162	0.993	0.965
p-value of test DEB = nonDEB	0.500	0.0258	0.622	0.552

**Notes:** Standard errors in parentheses. Standard errors are clustered at the community level. All regressions control for matched-pair fixed effects. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

We now examine the impact of DEB and non-DEB status on other pre-specified secondary outcomes. In Table 2.9 the outcome variables are related to schooling, and in Table 2.10 we examine assets, life satisfaction, and ART adherence using Equation (2.1). For none of these outcomes does DEB status have a positive effect that is large in magnitude or statistically significantly different from zero at conventional levels. The same holds true for non-DEB status. In the regression for the asset index, the coefficient on both DEB and non-DEB status is actually negative in sign and statistically significantly different from zero at the 10% level.

Table 2.9: FCC Impacts on Educational Outcomes

Group Indicators	(1) At school: household reported	(2) At school: observed registered	(3) At school: observed attend
<b>Panel A</b>			
DEB	-0.0161 (0.00984)	-0.0167 (0.0241)	-0.0160 (0.0518)
Non-DEB	0.00887 (0.0103)	0.000199 (0.0221)	0.00437 (0.0385)
R-squared	0.034	0.043	0.043
<b>Panel B</b>			
Treatment	-0.00325 (0.00846)	-0.00602 (0.0207)	-0.00404 (0.0373)
R-squared	0.034	0.043	0.043
Observations	10,094	4,351	1,526
Control Group Mean	0.864	0.429	0.316
p-value of test DEB = nonDEB	0.0280	0.410	0.681

**Notes:** Standard errors in parentheses. Standard errors are clustered at the community level. All regressions control for matched-pair fixed effects. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

## 2.5.2 Spillovers from DEB to non-DEB households

We are also interested in spillovers from direct beneficiary households (DEBs) to non-direct beneficiaries (non-DEBs). One key channel through which spillovers may occur is information: DEBs may share information with proximate non-DEBs. In addition, stigma may be a key mechanism, if reduced stigma by DEBs leads non-



Table 2.10: FCC Impacts on Assets, Life Satisfaction, and ART Adherence

Group Indicators	(1) Asset Index	(2) Life satis- faction ladder 0-10	(3) On ART	(4) High adherence of ART meds
<b>Panel A</b>				
DEB	-0.169* (0.0988)	0.0356 (0.169)	0.00503 (0.0195)	-0.0103 (0.0274)
Non-DEB	-0.157* (0.0856)	0.256 (0.175)	0.0162 (0.0214)	0.0208 (0.0334)
R-squared	0.206	0.094	0.046	0.080
<b>Panel B</b>				
Treatment	-0.163* (0.0872)	0.148 (0.166)	0.0103 (0.0150)	0.00443 (0.0224)
R-squared	0.206	0.093	0.045	0.079
Observations	4,240	4,575	768	736
Sample	Households	Adults	HIV+	HIV+
Control Group Mean	0.621	4.623	0.940	0.789
p-value of test DEB = nonDEB	0.845	0.0257	0.689	0.446

**Notes:** Robust standard errors in parentheses. Standard errors are clustered at the community level. All regressions control for matched-pair fixed effects. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

DEBs in proximity to them to be more willing to take up HIV testing. This analysis seeks evidence of spillovers via geographic proximity and social network ties.

Building on Equation (2.1), we will use the following equations to estimate spillovers, separately for social and geographic proximity:

$$\begin{aligned}
Y_{ijs} = & \alpha + \delta B_{ijs} + \sigma N_{ijs} \\
& + \nu \textit{Enroll}S_{ijs} \\
& + \omega S_{ijs} \\
& + \gamma_s + \epsilon_{ijs},
\end{aligned} \tag{2a}$$

$$\begin{aligned}
Y_{ijs} = & \alpha + \delta B_{ijs} + \sigma N_{ijs} \\
& + \mu \textit{EnrollDist1}_{ijs} + \zeta \textit{EnrollDist2}_{ijs} \\
& + \kappa \textit{Dist1}_{ijs} + \lambda \textit{Dist2}_{ijs} \\
& + \gamma_s + \epsilon_{ijs}.
\end{aligned} \tag{2b}$$

Compared to regression Equation (2.1), regression equations (2a) and (2b) add estimates of spillover impacts on households of being socially and geographically proximate to other households that were directly enrolled in the FCC program.  $\textit{Enroll}S_{ijs}$  is a measure of the extent to which members of one’s social network were randomly assigned to direct program enrollment.<sup>15</sup>  $\textit{EnrollDist1}_{ijs}$  is the number of directly-enrolled beneficiaries within a “close” radius of household  $i$ , while  $\textit{EnrollDist2}_{ijs}$  is similar but for direct beneficiaries in an “intermediate” distance.<sup>16</sup>

In each of these regression specifications, it is also important to control for variables representing the household’s general social connectedness and geographic proximity to other surveyed households, because we would expect that households with larger social networks or in more densely-populated neighborhoods to have more directly-enrolled individuals in their social networks or in geographic proximity. Failing to control for such variables would lead to biased estimates of the coefficients on  $\textit{Enroll}S_{ijs}$  in Equation (2a), and on  $\textit{EnrollDist1}_{ijs}$ , and  $\textit{EnrollDist2}_{ijs}$  in Equation (2b). There-

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<sup>15</sup>The number of social network members enrolled as direct beneficiaries is typically in the single digits. We specify this variable simply as the count (number) of social network members enrolled as direct beneficiaries. In the analysis sample, the number of social network members who are DEBs has mean 0.260 and standard deviation 0.781.

<sup>16</sup>The definition of “close” and “intermediate” distances are as follows, with mean and standard deviation of the number of DEBs: close 0-200 meters (mean 2.08, std.dev. 3.17), intermediate 200-500 meters (mean 6.37, std. dev. 7.41). “Far” distance would be the excluded or reference category.

fore, in Equation (2a), we control for  $S_{ijs}$ , a measure of the extent to which members of one’s social network are included in the survey sample. In Equation (2b), we control for  $Dist1_{ijs}$  (the number of surveyed households within a “close” radius of household  $i$ ) and  $Dist2_{ijs}$  (similar but for surveyed households in an “intermediate” distance).

In equations (2a) and (2b), the coefficients on  $EnrollS_{ijs}$ ,  $EnrollDist1_{ijs}$ , and  $EnrollDist2_{ijs}$  quantify particular types of spillover effects. The coefficient  $EnrollS_{ijs}$  isolates spillovers that operate through social network connections. It represents the impact of having additional social network members randomly assigned as DEBs.

Spillovers operating via geographic proximity are revealed in the coefficients on the interaction terms with the  $EnrollDist1_{ijs}$  and  $EnrollDist2_{ijs}$  variables.<sup>17</sup> The coefficient  $\mu$  on  $EnrollDist1_{ijs}$  is the impact of having more geographically close individuals randomly assigned as DEBs. We would expect this coefficient to be larger in magnitude than the coefficients  $\zeta$  the term corresponding to “intermediate” distance. These spillover coefficients are all credibly interpreted as causal effects. Because direct enrollment in FCC is randomly assigned, the extent to which households have directly-enrolled households in their social network or geographically proximate is also random.<sup>18</sup>

Hypothesis tests regarding spillovers from DEB to non-DEB households refer to coefficients  $\nu$ ,  $\mu$ , and  $\zeta$  in these regressions for the relevant outcome variable.

Regression results from the estimation of equations (2a) and (2b) are presented in Table 2.11. None of the coefficients representing spillovers (on the variables “Number of DEBs in Social Network”, “Number of DEBs within 0-200 meters”, and “Number of DEBs within 200-500 meters”) are large in magnitude or statistically significantly different from zero. These results provide no indication of substantial spillovers between DEB and non-DEB households leading to differences in HIV testing.

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<sup>17</sup>Measuring geographic spillovers in this manner corresponds to the widely-emulated method used in Miguel and Kremer (2004) to capture health spillovers of deworming in Kenya.

<sup>18</sup>It is reasonable to presume that spillover effects differ between households who themselves were and were not randomly assigned to direct FCC enrollment. In particular, we might expect spillover impacts to be larger for households not directly enrolled. We will also investigate such heterogeneity in the magnitude of spillovers. In exploratory analyses, we would estimate regression specifications that add interaction terms with the  $EnrollS_{ijs}$ ,  $EnrollDist1_{ijs}$  and  $EnrollDist2_{ijs}$  variables, on the one hand, with the indicators  $B_{ijs}$  and  $N_{ijs}$  on the other. A comparison of corresponding coefficients on the  $B_{ijs}$  and  $N_{ijs}$  interaction terms would reveal whether spillovers had a greater impact among the directly-enrolled compared to the non-directly-enrolled.

Table 2.11: The Spillover of FCC Impacts on HIV Testing

Variables	(1) HIV test	(2) HIV test: self-report	(3) HIV test: coupon use
<b>Panel A</b>			
DEB	0.0224 (0.0283)	0.00709 (0.0346)	-0.0359 (0.0356)
Non-DEB	0.0478 (0.0302)	0.0297 (0.0348)	0.0364 (0.0302)
# of DEBs in Social Network	0.00330 (0.0211)	0.00774 (0.0210)	-0.0292 (0.0177)
# of OVC HHs in Social Network	0.00273 (0.00536)	0.00147 (0.00575)	0.00869* (0.00454)
Observations	1,424	1,424	1,444
R-squared	0.051	0.050	0.072
Control Group Mean	0.723	0.630	0.280
p-value of test DEB = nonDEB	0.412	0.535	0.0150
<b>Panel B</b>			
DEB	0.0247 (0.0262)	0.0336 (0.0308)	-0.0337 (0.0309)
Non-DEB	0.0363 (0.0276)	0.0466 (0.0328)	0.0106 (0.0320)
# of DEBs in 0-200 meters	-0.00205 (0.00398)	-0.00288 (0.00454)	0.00438 (0.00345)
# of DEBs in 200-500 meters	0.000515 (0.00211)	8.96e-05 (0.00247)	-0.000496 (0.00245)
# of OVC HHs in 0-200 meters	0.00155 (0.00107)	0.00100 (0.00127)	0.000532 (0.00127)
# of OVC HHs in 200-500 meters	0.000453 (0.000565)	0.000951 (0.000748)	0.000614 (0.000677)
Observations	4,179	4,179	4,240
R-squared	0.034	0.035	0.059
Control Group Mean	0.723	0.626	0.264
p-value of test DEB = nonDEB	0.509	0.471	0.0140

**Notes:** Robust standard errors in parentheses. Standard errors are clustered at the community level. All regressions control for matched-pair fixed effects. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

### 2.5.3 Randomization Stage 3 Treatments

The purpose of the Randomization Stage 3 treatments is to understand the complementarity between the FCC program, on the one hand, and future public health information interventions and HIV testing incentives, on the other. They also help reveal the potential mechanisms behind our primary results above. That said, the main effect of the Randomization Stage 3 treatments themselves is also of interest.

The main effect of these treatments is estimated using the following modification of Equation (2.1):

$$\begin{aligned}
Y_{ijs} = & \alpha + \beta B_{ijs} + \lambda N_{ijs} \\
& + \rho \text{InfoHIV}_{ijs} + \tau \text{InfoART}_{ijs} + \theta \text{InfoHIV/ART}_{ijs} \\
& + \pi \text{Anti-Stigma}_{ijs} + \psi \text{HighTestPayment}_{ijs} \\
& + \gamma_s + \epsilon_{ijs}.
\end{aligned} \tag{3}$$

$Y_{ijs}$  is the post-treatment outcome for household  $i$  in community  $j$  in stratification cell (matched pair)  $s$ . The outcome variable for this analysis is the objective (coupon-redemption-based) measure of household HIV testing.  $B_{ijs}$ ,  $N_{ijs}$ ,  $\gamma_s$ , and  $\epsilon_{ijs}$  are as in previous regressions.

$\text{InfoHIV}_{ijs}$  is an indicator equal to one if a household was randomly assigned to receiving the treatment providing information on HIV/AIDS, and zero otherwise.  $\text{InfoART}_{ijs}$  and  $\text{Anti-Stigma}_{ijs}$  are defined similarly, but for the randomly-assigned ART information and anti-stigma treatments, respectively.  $\text{InfoART}_{ijs}$  is the indicator for receiving both the HIV and ART information treatments.  $\text{HighTestPayment}_{ijs}$  is an indicator for being offered the higher-value coupon for receiving an HIV test.

The coefficients  $\rho$ ,  $\tau$ ,  $\theta$ ,  $\pi$ , and  $\psi$  are the intent to treat (ITT) effects of household assignment to the corresponding treatment. These can be interpreted as causal effects because each is randomly assigned.

The hypothesis tests regarding the impact of the Randomization Stage 3 treatments refer to coefficients  $\rho$ ,  $\tau$ ,  $\theta$ ,  $\pi$ , and  $\psi$  in this regression.

Analyses of complementarity between the FCC program and the more targeted Randomization Stage 3 treatments are conducted using the following regression equa-

tion, which is a modification of Equation (3):

$$\begin{aligned}
Y_{ijs} = & \alpha + \beta B_{ijs} + \lambda N_{ijs} \\
& + \rho InfoHIV_{ijs} + \tau InfoART_{ijs} + \theta InfoHIV/ART_{ijs} \\
& + \pi Anti-Stigma_{ijs} + \psi HighTestPayment_{ijs} \\
& + \delta B_{ijs} \times InfoHIV_{ijs} + \varpi B_{ijs} \times InfoART_{ijs} + \xi B_{ijs} \times InfoHIV/ART_{ijs} \\
& + \omega B_{ijs} \times Anti-Stigma_{ijs} + \mu B_{ijs} \times HighTestPayment_{ijs} \\
& + \sigma N_{ijs} \times InfoHIV_{ijs} + \phi N_{ijs} \times InfoART_{ijs} + \eta N_{ijs} \times InfoHIV/ART_{ijs} \\
& + \nu N_{ijs} \times Anti-Stigma_{ijs} + \nu N_{ijs} \times HighTestPayment_{ijs} \\
& + \gamma_s + \epsilon_{ijs}.
\end{aligned} \tag{4}$$

This regression is similar to Equation (3), but adds interaction terms between  $B_{ijs}$  and each of the Randomization Stage 3 treatments, as well as interaction terms between  $N_{ijs}$  and each of the Randomization Stage 3 treatments. These interaction terms reveal whether the effects of the Randomization Stage 3 treatments differ for DEB and non-DEB households, compared to the effect in control communities. Because of the inclusion of these interaction terms, the coefficients  $\rho$ ,  $\tau$ ,  $\theta$ ,  $\pi$ , and  $\psi$  represent the difference between the ITT effects of assignment to the treatments *for households in control communities*.

The coefficients  $\delta$ ,  $\varpi$ ,  $\xi$ ,  $\omega$ , and  $\mu$  represent the difference in the ITT effect of the Randomization Stage 3 treatments *for DEB households*, compared to the effect for households in control communities. The hypothesis tests regarding how impacts of the Randomization Stage 3 treatments differ for DEB households refer to these coefficients.

The coefficients  $\sigma$ ,  $\phi$ ,  $\eta$ ,  $\nu$ , and  $\nu$  represent the difference in the ITT effect of the Randomization Stage 3 treatments *for non-DEB households*, compared to the effect for households in control communities. The hypothesis tests regarding how impacts of the Randomization Stage 3 treatments differ for non-DEB households refer to these coefficients.

Results from estimating equations (3) and (4) are in Table 2.12. Estimation of the average effects across the full sample (Equation (3), Column 1) reveals that only the high-value coupon has an effect on HIV testing rates that is statistically significant

at conventional levels. The effect amounts to 6.9 percentage points, on top of the control group rate of 28.4%.

Estimation of differential effects of the Stage 3 treatments across DEB and non-DEB treatment groups (Equation (4), Column 2) helps provide explanations for the effects found in prior results tables. The coefficient on the DEB main effect (top row of Column 2) represents the impact of DEB status for individuals who did not get any of the Stage 3 treatments. The coefficient is negative, large in magnitude (10.7 percentage points), and statistically significant at the 1% level. This result reveals that DEB status actually substantially *reduces* HIV testing rates.

Coefficients on the interaction terms between DEB status and the Stage 3 treatments (row 9-13 of Column 2) indicate how the Stage 3 treatments modify the main effect of DEB status. All of the interaction term coefficients are positive, and most are large in magnitude and statistically significantly different from zero at conventional levels. Providing HIV-related information, counteracting concerns about HIV-related stigma, and providing higher financial incentives all make the impact of DEB status on HIV testing more positive. These effects are comparable to the magnitude to that of the main effect of DEB status; all these Stage 3 treatments therefore can be viewed as counteracting the negative effect of DEB status on HIV testing. These effects are also all similar in magnitude to the effect of providing additional financial incentives (an additional 50 MZN) to get an HIV test.

The exception to this pattern is the coefficient on the interaction term with the combined HIV and ART information treatment, which is much smaller in magnitude and not statistically significantly different from zero at conventional levels. This is somewhat hard to explain. It is possible that providing too much information to respondents reduces the effectiveness of all information provided, perhaps by causing lapses in respondents' concentration or attention.

The main effects of the Stage 3 treatments in Column (2) (row 4-8) represent impacts in control communities. All of these effects are negative, small in magnitude, and not statistically significantly different from zero. The exception is the coefficient on the anti-stigma treatment, which is significant at the 10% level. It is possible that in control communities the anti-stigma treatment actually makes stigma concerns more salient, making people more reticent about getting tested.

Coefficients on the interaction terms between non-DEB status and the Stage 3 treatments (the last rows of coefficients in Column 2) indicate how the Stage 3 treatments modify the main effect of non-DEB status. Consistent with the non-DEB

Table 2.12a: Randomization Stage 3 Treatment Effects - *part 1*

Group Indicators	(1) HIV test: coupon use	(2) HIV test: coupon use
DEB	-0.0217 (0.0179)	-0.107*** (0.0381)
Non-DEB	0.0246 (0.0181)	0.0155 (0.0424)
Anti-Stigma	0.00654 (0.0202)	-0.0529* (0.0269)
HIV Information	-0.00113 (0.0220)	-0.0326 (0.0304)
ART Information	-0.0112 (0.0239)	-0.0334 (0.0319)
High-Value Coupon	0.0693** (0.0272)	0.0304 (0.0443)
HIV and ART Information	-0.0342 (0.0223)	-0.0229 (0.0352)
DEB×Anti-Stigma		0.140*** (0.0472)
DEB×HIV Information		0.108** (0.0515)
DEB×ART Information		0.131** (0.0547)
DEB×High-Value Coupon		0.123** (0.0588)
DEB×HIV and ART Information		0.000591 (0.0543)
Non-DEB×Anti-Stigma		0.0928* (0.0478)
Non-DEB×HIV Information		0.0165 (0.0566)
Non-DEB×ART Information		-0.0479 (0.0637)
Non-DEB×High-Value Coupon		0.0299 (0.0710)
Non-DEB×HIV and ART Information		-0.0471 (0.0626)
Observations	4,240	4,240
R-squared	0.062	0.066
FCC and Stage3 Control Group Mean	0.284	0.284



Table 2.12b: Randomization Stage 3 Treatment Effects - *part 2*

Group Indicators	(3) HIV test: coupon use	(4) HIV test: coupon use
Treatment	0.00165 (0.0157)	-0.0458 (0.0344)
Anti-Stigma	0.00673 (0.0202)	-0.0530* (0.0269)
HIV Information	-0.000151 (0.0220)	-0.0326 (0.0304)
ART Information	-0.0118 (0.0239)	-0.0334 (0.0318)
High-Value Coupon	0.0691** (0.0272)	0.0304 (0.0443)
HIV and ART Information	-0.0340 (0.0224)	-0.0228 (0.0351)
Treatment×Anti-Stigma		0.117*** (0.0374)
Treatment×HIV Information		0.0636 (0.0432)
Treatment×ART Information		0.0424 (0.0478)
Treatment×High-Value Coupon		0.0761 (0.0538)
Treatment×HIV and ART Information		-0.0235 (0.0451)
Observations	4,240	4,240
R-squared	0.060	0.063
FCC and Stage3 Control Group Mean	0.284	0.284

**Notes:** Standard errors in parentheses. Standard errors are clustered at the community level. All regressions control for matched-pair fixed effects. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

treatment being less intensive than the DEB treatment, all of these interaction term coefficients are closer to zero compared to the corresponding interaction terms with the DEB treatment. Only one is statistically significantly different from zero: the coefficient on the interaction term with the anti-stigma treatment, which is positive and significant at the 10% level.

All told, we view the results from the analysis of the Stage 3 treatments as providing additional support for the interpretation of our prior results. The FCC program’s modest effects on HIV testing are likely due to the unintended consequence that the program led to increases in stigmatizing attitudes, and had no effect on HIV-related information.

The pattern of impacts of Stage 3 treatments additionally bolster the idea that the FCC program had important deficiencies in providing HIV information and in countering stigma concerns. The Stage 3 treatments providing HIV-related information and countering concerns about HIV-related stigma make the impact of the FCC program on HIV testing more positive. This positive effect is off a base of a substantial *negative* impact of the program on testing among those who got none of the Stage 3 treatments.

## 2.6 Conclusion

We study the impacts of a widespread community health program on HIV testing in Mozambique. We exploit a multilevel randomized treatment design to identify causal effects. We find that the program *Força à Comunidade e Crianças* (FCC, “Strengthening Communities and Children”) had only modest positive effects on HIV testing rates. These effects are substantially smaller than the effects predicted in advance by expert forecasters. Rich survey data on secondary outcomes, alongside additional household-level treatments, helps shed light on underlying mechanisms. These additional analyses suggest that the program’s modest impacts are due to the fact that it led to misinformation about HIV, and worsened HIV-related stigmatizing attitudes. Consistent with the increase in HIV-related stigmatizing attitudes, we find that the treatment also leads to a variety of safer sexual behaviors, including a reduction in the number of recent sexual partners.

Our interpretation that worsened information and stigma are behind the modest impact of the FCC program on testing is bolstered by additional treatments we administer at the household level. These additional treatments providing HIV-related

information and countering HIV-related stigma concerns make the treatment effects of the FCC program on HIV testing rates more positive, suggesting that the FCC program was deficient in these areas.

This study provides a rare glimpse into the impacts and mechanisms of a widespread community-level program seeking to raise HIV testing. Our results point to a thus-far neglected possibility: programs seeking to raise HIV testing may fail due to deficiencies in information delivery and in counteracting HIV-related stigma. Indeed, efforts such as the FCC program may inadvertently worsen stigmatizing attitudes. From a policy standpoint, it is important to know that PEPFAR programs may not currently be delivering the gains in HIV testing as currently structured. Our results point to deficiencies in the areas of HIV knowledge and HIV-related stigmatizing attitudes. It is important to study what is going on with PEPFAR OVC programs in more detail to get a better sense of what is leading to worsened information and worse stigmatizing attitudes.

These results suggest priority directions for future research. A key question is what exactly led the program to worsen HIV-related stigmatizing attitudes. It is possible that the program led to misinformation about HIV on some dimensions that we do not measure. This may be consistent with our finding that the FCC program led people to have fewer sexual partners. The program may have magnified beliefs about HIV's negative consequences, leading them to safer sexual behavior, as well as more stigmatizing attitudes. Another profitable area for future exploration is in ways to better improve people's knowledge about HIV. There are clearly better and worse ways to improve knowledge, as evidenced by our own results: the FCC program did a poor job of it, while our own simple post-survey treatments did have positive effects. Future work should seek to pursue these and other related avenues for research.

## CHAPTER III

# The Value of Political Connections for Firms the Case of Government-Official Outside Directors in China

### 3.1 Introduction

Political connections are special resources for firms across the world. They can be obtained in various ways under different political institutions. In some firms, the major shareholders have family ties to politicians in power; in others, people from the management hold public positions themselves. Many firms can also build such connections by supplying political contributions. Since the pioneering work of Fisman (2001), researchers have been trying to determine and quantify the value of political connections to firms.

Some studies document that gaining (or losing) political connections instantly raises (or lowers) a firm's stock price (Faccio, 2006; He, Wan and Zhou, 2014; Luechinger and Moser, 2014). Politically-connected firms seem to receive more subsidies (Qin, 2013), sign more government contracts (Amore and Bennedsen, 2013; Ağca and Igan, 2015), enjoy more bailouts (Faccio, Masulis and McConnell, 2006), obtain more IPO approvals (Liu, Tang and Tian, 2013), and acquire cheaper bank loans (Houston et al., 2014; Infante and Piazza, 2014).

Firms with political connections may systematically differ from those without such connections. Direct comparisons between connected and unconnected firms, therefore, often face endogeneity concerns. Researchers usually explore exogenous changes in political connections to identify their values. Examples of popular exogenous changes include the sudden rise and fall of connected politicians and unexpected election

results (Fisman, 2001; Fan, Rui and Zhao, 2006; Jayachandran, 2006; Ferguson and Voth, 2008; Faccio and Parsley, 2009).

There remain unsolved concerns in these identification strategies, however. First, although the events mentioned above are mostly unexpected, they are not usually “clean” shocks: These big events affect firms’ political connections but can also shock firms in many other ways. If an event affects connected and unconnected firms differently through other channels, the estimated effect of the event is a mixture of “the value of political connections” and these other unknown factors. Second, by focusing on influential politicians or important elections, these studies consider very high-level, salient political connections. Firms with such connections are special, and findings from this small selective sample do not support a general narrative.

From the summaries above, obtaining a credible and generalizable evaluation of political connections requires large-scale, clean, exogenous shocks that affect firms’ general political connections. A recent policy change in China is a good example of this kind.

As a part of its anti-corruption campaign, the Chinese central government announced a new policy in October 2013 to restrict government officials from working in firms. At that time, the position in firms that government officials held most often is the outside director position. After the new policy was announced, government-official outside directors left firms gradually. Firms previously having such outside directors were affected. These affected firms exogenously lost a certain type of political connection: government-official outside directors.

Taking this policy as an exogenous shock to firms, this paper employs difference-in-difference (DID) and matching methods to estimate the value of government-officials outside directors for listed non-state-controlled firms. In the 12 months following the policy change, the affected firms exhibit an 8 percentage point lower stock return on average. The change in stock returns reflects the market-perceived value of this type of political connections. Further analyses show that the value of such political connections varies substantially across firms. There is also suggestive evidence that the loss of government-official outside directors has real effects on firms’ business performances, as are reflected in the financial reports.

This study contributes to the discussion of the value of political connections. Several properties of the particular policy change help us fill gaps in the existing literature. First, it is a policy that was announced suddenly and enforced effectively throughout the country. This gives us exogenous variations at a large scale to iden-

tify causality. Second, this new policy specifically targeted the connections between firms and government officials. The shock is clean in the sense that it affects firms only through the channel of government official-firm connections. This allows us to relate the effect of this policy to the value of political connections, but not to other factors. Last, this policy enables us to study a new form of political connection: employing government officials as outside directors. This type of political connection exists widely but has not been well understood.

The findings on stock return changes in this study extend findings from Fisman (2001), Jayachandran (2006), Faccio and Parsley (2009), and others. These previous works have documented that the shocks in connections with very influential politicians lead to turbulence a firm’s stock return in a few-day window. This paper further shows that shocks in connections with general government officials can also affect stock returns, and that the effect can last in a longer time horizon - it appears gradually in several months and persists for up to a year.

Several research papers have studied this policy change from different perspectives. Fan (2016) analyzes a sample of all mainboard listed firms in China, which includes state-controlled firms and are, in general, larger and older than firms in our sample. The “government officials” are defined in a broader way than those in this paper, some of whom are not directly regulated by the new policy. Fan (2016) finds that a firm’s stock return was affected when a government-official board member *actually* left the firm, while this current paper shows that the policy effect appeared as soon as the policy was announced and that whether or not a government official actually left the firm does not change the estimated policy effect. Liu, Lin and Wu (2016) and Tang et al. (2016) study the short-term effect of the policy. Using all listed firms in China, they both find that firms with government-official outside directors had negative cumulative abnormal returns (CAR) in a few-day window. They also document that the policy effect varies with industry and region. This paper does not find similar patterns in the sample of non-state-controlled firms in the longer time horizon.

## **3.2 Institutional Backgrounds and the Policy Change**

### **3.2.1 Outside Directors in Publicly-Traded Companies in China**

Outside directors are board members who are independent of the firms they serve. An outside director should not be a shareholder or employee of the firm, or a family member of a major shareholder or employee. Many countries have required publicly-traded companies to have a certain number of outside directors on the board. An outside director position is designed to be a part-time job. They do not involve themselves in the everyday businesses of the firm. The main duty of an outside director is to provide independent opinions on major firm events, such as correlative transactions and appointment and removal of managers.

China began to build its Outside Director System in 2001, when the China Securities Regulatory Commission (CSRC) released *The Guidance to Build Outside Director System in Publicly Traded Companies*. CSRC requires that all companies traded in the Shanghai or Shenzhen Stock Exchanges should have at least one-third of their board members to be outside directors and that at least one outside director should be an accounting professional. In practice, other popular choices for outside directors include lawyers, professors, and retired government officials. Each individual can work as outside directors for no more than 5 firms at the same time and the maximum appointment in one firm should not exceed six years.

### **3.2.2 Government-Official Outside Directors**

Government officials in China have always been popular candidates for outside director positions. In theory, under a political system lacking transparency, they could benefit firms with their experience in dealing with political issues and their powers in influencing government decisions. Having a government official on the board also send a signal to the market that the firm has a good relationship with the government. This practice is also greatly welcomed by government officials themselves, because this is a legal and convenient way they could receive monetary compensation from firms.

### **3.2.3 Old Regulations**

The practice that government officials work as outside directors has long been subject to regulations in China. Two regulations were already in force by the time

the new policy was announced in 2013: *The Civil Servant Law*, and an official order issued by Central Commission for Discipline Inspection (CCDI).

*The Civil Servant Law* came into effect in 2006. By this law, when government officials are in office, they should not involve themselves in any for-profit business. Within 3 years after leaving office, they should not work for any firm in the same industry and administrative area as their previous public positions were in.

The CCDI order was issued in 2008. It put extra restrictions on central-disciplined officials'<sup>1</sup> ability to work as outside directors. Hereafter, we call the restrictions put by the CCDI order Old Restrictions. Column (1) in Table 3.1 summarizes the key elements. Under the Old Restrictions, government officials could work for multiple firms and could receive compensation from firms.

Table 3.1: Comparison between the Old Restrictions and the New Restrictions

	Old Restrictions	New Restrictions
Case approved by	The government agency one previously held office in and ODCCCPC	The government agency one previously held office in; ODCCCPC for central-disciplined officials, OD of the local committee of CPC for other officials.
Monetary compensation	No restrictions	Should not receive any monetary compensation
Number of firms to work for	No restrictions	No more than one
Age limit to work in firms	No restrictions	Under 70

Column (1) in Table 3.2 lists the restrictions government officials were facing in different scenarios before the policy change in 2013. In summary, government officials who are currently in office are not allowed to take jobs in firms. Government officials who have left their positions in the government could work for firms that were unrelated to their previous public positions immediately after leaving office. They became eligible to work for any firms after having left office for 3 years. Only central-disciplined officials faced some further restrictions.

<sup>1</sup>Central-disciplined officials are high-rank government officials whose appointments and dismissals are directly controlled by the central government. They include heads of national ministries, provinces and major cities.



Table 3.2: Restrictions before and after the Policy Change

Scenarios	Description of Scenarios	Before the Policy Change	After the Policy Change
I	In office, work for any firms	Not allowed	Not allowed
II	Within 3 years after leaving office, work for firms related to the previous public position	Not allowed	Not allowed
	Within 3 years after leaving office, work for firms unrelated to previous public positions		
III	– Central-disciplined officials	Old Restrictions	New Restrictions
IV	– Other public employees	No restriction	New Restrictions
	Left office for more than 3 years, work for any firms		
V	– Central-disciplined officials	Old Restrictions	New Restrictions
VI	– Other public employees	No restriction	New Restrictions

### 3.2.4 The New Policy

On October 19, 2013, the Organization Department (OD) of the Central Committee of the Communist Party of China (CCCPC) announced a package of new regulations concerning government officials working at firms. The new CD regulation package is the policy change of interest in this study. The restrictions enacted by this new policy, hereafter New Restrictions, are stricter than the Old Restrictions. See Column (2) in Table 3.1. The New Restrictions are imposed on not only central-disciplined officials but also all other government officials. Table 3.2 compares the restrictions under different situations before and after the policy change.

The new policy greatly tightened restrictions on working for firms and expanded the number of constrained persons from a few thousand high-rank officials to all the millions of government officials. By the new policy, none are allowed to receive any compensation from firms, which is essential to cut the benefit chain between government officials and firms.

There are two caveats. First, in theory, former government officials could work for firms not only as outside directors, but also as full-time managers, before and

after the policy change. But working full time in firms is subject to even stricter regulations. In general, if government officials choose to work full time in firms after leaving office, they will lose their administrative ranks and any associated pensions and benefits. Therefore, even before the policy change, being an outside director was the only practical choice for government officials who wanted to work in firms. Such cases are rare and beyond the scope of this paper. Second, *the Civil Servant Law*, CCDI order and the new OD policy regulate not only government officials, but also state-owned-enterprise managers and public institute directors. Of all three types of persons, however, only government officials hold or have held positions in government agencies and, thus, have public power. Their sitting on the board brings a firm “political connection” in its narrow sense. This study will only focus on the value of government officials on the board.

### **3.2.5 Policy Enforcement and Affected Firms**

The new policy was strongly enforced as part of the nationwide anti-corruption campaign. Before the policy change, many government officials facing scenario III - VI in Table 3.2 worked in firms as outside directors. Although the new policy specified conditions under which government officials can stay in firms, it effectively removed all government officials from the boards under high political pressure. In the fierce “War Against Corruptions,” government officials try to stay away from any suspicion of “being corruptive”. Resigning from the firms, therefore, showed their support for the new policy and their loyalty to the anti-corruption campaign. Moreover, the new policy prohibits government officials from receiving compensation, which leaves them with no economic incentives to stay.

Firms that previously had government-official board members were affected by this new policy. They lost their political connections through outside directors. In practice, immediately after the policy change, the government officials make announcements of their intent to resign from the outside director positions. Firms usually accept their resignations but require them to keep performing their duties as outside directors until the next board election. The policy-makers allow this compromise practice to avoid interrupting firms’ operations. In practice, about half of the government officials left the board within one year after resigning, while the others stayed longer.

### 3.3 Data

The sample in this paper consists of private companies listed on the Shanghai Stock Exchange and the Shenzhen Stock Exchange. From the 2458 companies listed by October 19, 2013, we dropped 1094 companies whose actual controllers were government agencies or public institutions. We further dropped 2 companies that had been listed for less than one year by the policy change. The remaining 1362 firms constitute the main analysis sample. All firm information, stock market returns, firm performance variables, and profiles of board members are exported from Wind Database.

#### 3.3.1 Construction of Treatment Variable

There were 3449 outside directors in all 1362 sample firms on the policy change day. Their profiles are available in the 2013 annual reports. A profile usually includes a board member's birth year, education, professional titles, major social positions and a brief history of employment. Since government officials in office were not allowed to take jobs in firms by law, all government officials in the sample had already left government agencies. By manually reading all profiles, we can identify government officials from all outside directors. A person is identified as a government official if he or she

1. has been formally employed in a government agency, has administrative rank and
2. retired from the government agency, or been appointed to a government-run social organization <sup>2</sup> after leaving the government agency.

Following the rules above, 260 out of the 3449 outside directors are identified as government officials. The 260 government officials worked in 261 firms (out of 1362 sample firms). Note that one person could work in multiple firms and one firm could have multiple government-official outside directors. These 261 firms constitute the treatment group. The remaining 1101 firms constitute the control group. The board structures are summarized in Table 3.3. On average, each treatment firm had 3.18 outside directors, of which 1.21 were government officials. Each control firm had 3.06 outside directors and, by construction, had no government officials on its board.

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<sup>2</sup>Examples are industry associations, academy societies.

Table 3.3: Board Structure of the Treatment and the Control Firms

	Treatment Firms			Control Firms		
	Mean	Min	Max	Mean	Min	Max
# of outside directors	3.18 (0.03)	1	5	3.06 (0.01)	2	6
of which						
# of government officials	1.21 (0.03)	1	4			
Number of observations		261			1,101	

**Notes:** Robust standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

### 3.3.2 Summary Statistics

Table 3.4 compares the characteristics of the government officials and the other outside directors. Not surprisingly, since they are mostly retirees from public positions, the government officials are older. The government officials are significantly less likely to be females. They are also less likely to hold graduate degrees compared to the other outside directors, whose main occupations are usually lawyers, accountants or professors, etc. The government-official outside directors are usually influential officials, at least locally. Over 90 percent of them have administrative ranks that are no lower than that of the head of a county. More than 40 percent have higher ranks than that of the head of a municipality.

Table 3.5 compares the baseline characteristics of the treatment and the control firms. The treatment firms are, on average, larger than the control ones. They have significantly more assets, higher revenues and higher market values. The treatment and the control firms do not systematically differ in other dimensions like profitability (measured by profit margin, ROA, ROE), growth momentum (measured by revenue growth rate and past 12-month stock return), or investment behaviors (measured by R&D expense and investment).

## 3.4 The Policy Effect on Stock Market Performances

Following Fisman (2001), Faccio (2006) and many others, we use the stock return as the outcome variable to study the value of political connections. The stock return has several merits that serve the goal of this paper well. First, the stock prices reflex overall firm performance. Their fluctuations, therefore, could in principle capture

Table 3.4: Outside Director Characteristics

Characteristics	Government Officials Mean	Non- Government Officials Mean	Difference (std. err.)
Age	64.76 (0.41)	51.59 (0.16)	13.17*** (0.58)
Female	0.09 (0.02)	0.17 (0.01)	-0.08*** (0.02)
Tenure as outside director	3.17 (0.11)	3.06 (0.03)	0.11 (0.12)
Education			
<i>College degree or higher</i>	0.83 (0.02)	0.94 (0.00)	-0.11*** (0.02)
<i>Graduate degree or higher</i>	0.32 (0.03)	0.67 (0.01)	-0.35*** (0.03)
Administrative rank			
<i>County level or higher</i>	0.92 (0.02)		
<i>Municipality level or higher</i>	0.43 (0.03)		
Observations	260	3,189	

**Notes:** Robust standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$  for the last column. *Tenure as outside director* is defined on (person  $\times$  firm) pairs. There are 316 (government official  $\times$  firm) pairs and 3,888 (non-government official  $\times$  firm) pairs. Standard errors in the Tenure as an outside director row are clustered at the individual level.

Table 3.5: Baseline Firm Characteristics

	Mean			Median		
	Treat	Control	Diff.	Treat	Control	Diff.
Total Assets	4,398 (548.7)	3,105 (176.1)	1,293*** (449.5)	1,923 (164.7)	1,543 (56.58)	380.6*** (142.7)
Tangible Assets	1,470 (123.0)	1,268 (47.65)	201.2* (114.7)	955.0 (55.76)	852.5 (27.46)	102.5* (60.74)
Debt-to-Asset Ratio	38.95 (1.39)	39.35 (2.07)	-0.41 (4.31)	35.65 (2.31)	31.00 (1.02)	4.657** (2.35)
Book-to-Market Ratio	44.80 (1.31)	44.72 (0.69)	0.08 (1.56)	43.30 (1.57)	43.24 (0.72)	0.06 (1.65)
Operating Revenue	2,762 (400.1)	2,029 (159.1)	732.8* (380.3)	1,014 (90.97)	795.0 (39.33)	219.1** (92.93)
Revenue Growth	-81.41 (3.73)	-36.44 (34.46)	-44.97 (70.73)	-93.55 (1.78)	-91.31 (0.90)	-2.242 (2.00)
R&D Expenditures	61.35 (14.12)	44.72 (2.43)	16.63* (8.48)	21.53 (2.73)	22.27 (1.11)	-0.74 (2.53)
Net Investment	101.7 (24.72)	135.2 (16.82)	-33.53 (36.59)	26.09 (5.70)	32.12 (2.96)	-6.030 (6.61)
Profit Margin	-9.34 (0.84)	-10.06 (2.73)	0.72 (5.62)	-8.13 (0.79)	-8.61 (0.39)	0.48 (0.89)
Return on Assets (ROA)	5.22 (0.37)	5.56 (0.48)	-0.34 (1.01)	4.32 (0.34)	4.75 (0.18)	-0.44 (0.40)
Return on Equity (ROE)	7.76 (0.60)	7.69 (0.34)	0.08 (0.75)	7.14 (0.46)	7.26 (0.23)	-0.12 (0.53)
Market Value	6,155 (574.6)	4,980 (228.4)	1,175** (546.1)	3,648 (278.7)	3,054 (97.19)	594.0** (236.9)
12-Month Stock Return	37.18 (3.44)	36.82 (1.83)	0.357 (4.10)	23.08 (3.65)	21.22 (1.46)	1.865 (3.21)
Observations	261	1,101		261	1,101	

**Notes:** The Total Assets, Tangible Assets, Operating Revenue, R&D Expenditures, and Net Investment are measured by million RMB (Chinese *yuan*). The Debt-to-Asset Ratio, Book-to-Market Ratio, Revenue Growth, Profit Margin, ROA, and ROE are measured in percentage points. All variables above are reported in or derived from the 2012 Annual Financial Reports of the firms. The Market Value is measured by the end of September 2013, in million RMB. The 12-Month Stock Return is the stock return between October 2012 and September 2013 (inclusive), measured in percentage points. The Book-to-Market Ratio is constructed following Fama and French (1993). For the two columns reporting differences, standard errors are clustered at the industry levels. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

policy effects on any aspect of the firm. Second, stock prices are sensitive to information as well as actual events. The new policy was announced at one time, but firms reacted at different times. Stock returns can capture any effect after the policy was announced, even before a firm had enforced the policy. Moreover, stock prices are available at high frequency, which allows us to study the dynamics of the policy effect.

Figure 3.1 compares the equal-weighted mean market returns of the treatment firms and the control firms in a 24-month window covering the policy change. We define the policy change month, October 2013, as month 0, the month prior to the policy change month as month -1, the month after the policy change month as month 1, and so on. During the analysis period, the stock market in China has experienced a fast growth as a whole. The treatment and the control groups co-moved before the policy change. Their performances began to diverge after the new policy was announced. The discrepancy between the two groups evolved to more than 10 percentage point by month 6 and stabilized thereafter.

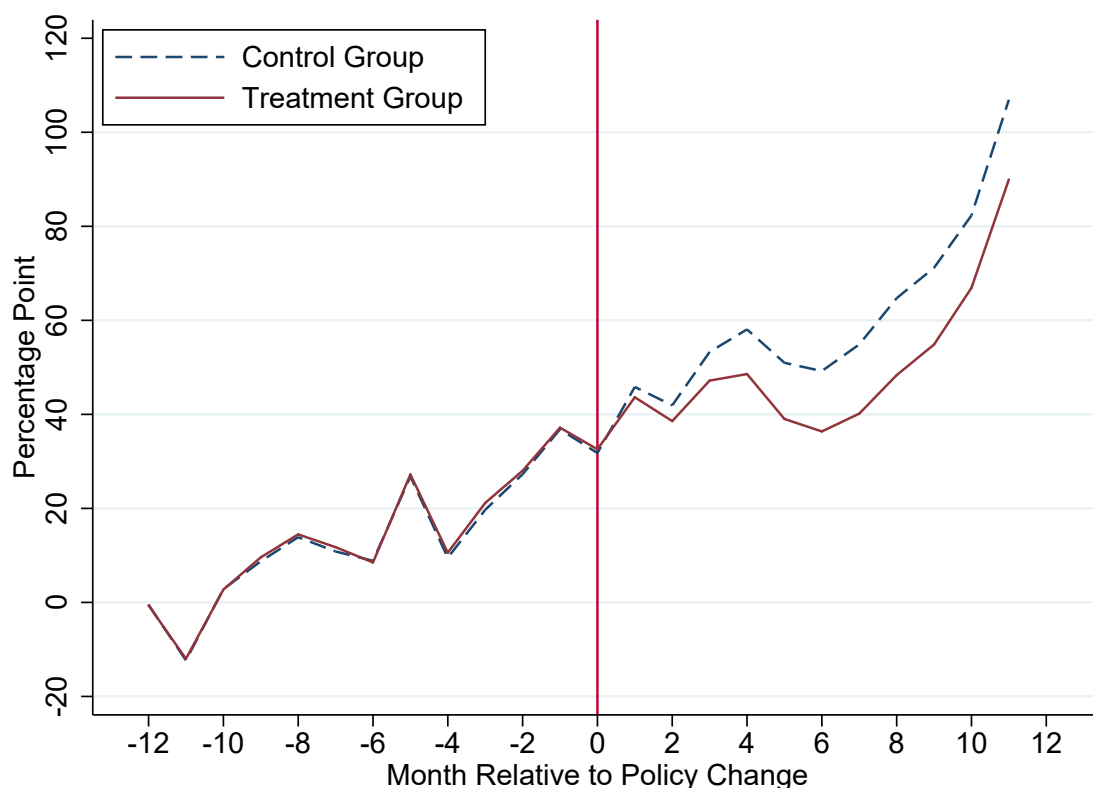


Figure 3.1: Equal-Weighted Mean Market Returns since Month -12

### 3.4.1 Difference-in-Difference Approach

The close co-movements of the two groups in the pre-treatment period suggest a valid difference-in-difference (DID) setup. As the first step to obtain the overall treatment effect in the long term, we stack the 24 months (from month -12 to month 11) into two 12-month-long periods: one pre-treatment period and one post-treatment period. The following regression is estimated with data from the two periods:

$$Y_{it} = \alpha + \beta(T_i \times P_t) + \delta T_i + \rho P_t + \gamma \mathbf{X}_i + \epsilon_{it} \quad (3.1)$$

The dependent variable  $Y_{it}$  is firm  $i$ 's stock return in a 12-month-long period  $t$ . In the pre-treatment period ( $t = 0$ ), it is the stock return from the beginning of month -12 to the end of month -1. In the post-treatment period ( $t = 1$ ), it is the stock return from the beginning of month 0 to the end of month 11.  $T_i$  is a treatment indicator that takes value 1 if and only if firm  $i$  is in the treatment group.  $P_t$  is a post-treatment indicator that takes value 1 if and only if the period is after the policy change. The coefficient on the interaction of treatment and post,  $\beta$ , is the DID estimate of the policy effect.  $\mathbf{X}_i$  is a vector of predetermined controls. In regressions with firm-fixed effects,  $T_i$  is absorbed by firm-fixed effects and  $\mathbf{X}_i$  is replaced with a set of firm indicators. Because there are only two periods in the baseline specification, time-fixed effects are not included in the regression.

Column (1) in Table 3.6 corresponds to Figure 3.1. The estimated treatment effect on the 12-month return is -12.94 percentage point. Column (2) controls for some predetermined characteristics instead of firm-fixed effects and brings us very similar estimates, -12.62 percentage points.

Column (3)-(4) redo the practice in Column (1)-(2) but shorten the analysis period to month -6 through month 5. The dependent variable in Column (3)-(4) becomes the 6-month stock return instead of the 12-month stock return. Analogously, regressions in Column (3)-(4) use two periods: one pre-treatment period and one post-treatment period. In the pre-treatment period ( $t = 0$ ), the dependent variable is the stock return from the beginning of month -6 to the end of month -1. In the post-treatment period ( $t = 1$ ), it is the stock return from the beginning of month 0 to the end of month 5. These point estimates of the treatment effect on the 6-month-return are close to those on the 12-month return, which suggests that most of the treatment effect arose during the first 6 months after treatment.



Table 3.6: Difference-in-Difference Regressions on Stock Returns

Variables	(1) 12-Month Return	(2) 12-Month Return	(3) 6-Month Return	(4) 6-Month Return	(5) Monthly Return	(6) Monthly Return
Treat $\times$ After	-12.94** (5.40)	-12.62** (4.29)	-10.54* (5.20)	-10.03*** (2.66)		
Treat $\times$ M0					0.28 (0.82)	0.30 (0.76)
Treat $\times$ M1					-2.13** (0.76)	-2.01** (0.88)
Treat $\times$ M2					-1.01 (0.56)	-0.98* (0.53)
Treat $\times$ M3					-1.93** (0.75)	-2.12** (0.66)
Treat $\times$ M4					-2.17*** (0.62)	-2.13*** (0.57)
Treat $\times$ M5					-1.33 (0.94)	-1.22 (1.00)
Treat $\times$ M6					-0.87 (0.57)	-0.75 (0.61)
Treat $\times$ M7					-1.06** (0.39)	-1.20** (0.41)
Treat $\times$ M8					-0.60 (0.69)	-0.56 (0.65)
Treat $\times$ M9					0.16 (0.87)	0.37 (0.89)
Treat $\times$ M10					0.62 (0.94)	0.62 (0.88)
Treat $\times$ M11					0.19 (0.56)	0.16 (0.61)
Controls		X		X		X
Outcome Mean	45.88	45.82	22.37	22.17	3.524	3.453
Observations	2,620	2,512	2,620	2,490	32,923	31,282
R-squared	0.42	0.13	0.48	0.15	0.320	0.33

**Notes:** Standard errors in parentheses. Standard errors are clustered at the industry level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Control variables are Market Value, 12-Month Return, Debt-to-Asset Ratio, Book-to-Market Ratio, and industry fixed effects. Market Value, 12-Month Return and Debt-to-Asset Ratio are measured by the end of Month -13 (September 2012). Book-to-Market Ratio is measured by the end of Month -16 (June 2012), because book value is only available on semiyearly reports and yearly reports (every June and December). The post-policy change indicator is controlled for in Column (1)-(4). Firm fixed-effects are included in Column (1) (3) and (5), while Column (2) (4) and (6) include the treatment indicator. Time fixed-effects are included in Column (6).

To break down the treatment effect into each individual month, Column (5)-(6) estimate a modified version of Equation (3.1).

$$Y_{it}^{monthly} = \alpha + \sum_{\tau} \beta_{\tau}(T_i \times P_t \times \mathbb{I}_{(t=\tau)}) + \delta T_i + \sum_{\tau} \rho_{\tau} \mathbb{I}_{(t=\tau)} + \gamma \mathbf{X}_i + \epsilon_{it} \quad (3.2)$$

The dependent variable,  $Y_{it}^{monthly}$ , in Equation (3.2) is the monthly stock return of firm  $i$  in month  $t$ . In practice, data from month -12 to month 11 (24 months in total) are used. Month-fixed effects are included in the equation to control for time trend. Each post-treatment month is interacted with the treatment indicator to decompose the policy effect into each individual month. The coefficient  $\beta_{\tau}$  is the treatment effect that arose during month  $\tau$ . In theory, the sum of  $\beta_{\tau}$ 's in Equation (3.2) is equal to the  $\beta$  in Equation (3.1) when other things equal.

From Column (5)-(6), we find that the treatment effect accumulated steadily during the first 7 months after the policy change, at a rate of 1 to 2 percentage point per month. The stock return discrepancy between the treatment and control group remained for at least another 4 months thereafter. Note that three important control variables, *Market Value*, *Book-to-Market Ratio* and *Momentum*, i.e. the past 12-month stock return, are endogenous to stock returns by construction. In the regressions, control variables are measured by the end of month -13<sup>3</sup>. All controls are predetermined for the analysis period.

### 3.4.2 Matching

The matching approach adopted in this paper is motivated by the idea of Buy-and-Hold-Abnormal-Return (BHAR) in corporate finance literature. BHAR, in its simplest form, equals to the return on a buy-and-hold investment in the treatment firm less the return on a buy-and-hold investment in a matched control firm. The use of BHAR in long-run event study is advocated by Barber and Lyon (1997), because it effectively alleviates new listing bias, rebalancing bias, and skewness bias. BHAR method also has the merit of closely resembling the investors' behaviors in the stock market. The mean BHAR over multiple treatment firms can be interpreted as a

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<sup>3</sup>*Book-to-Market Ratio* is measured by the end of month -16 (June 2012) because it can be measured only when the annual report or the semi-annual report is available.

matching estimator of the average treatment effect on the treated (ATET) on buy-and-hold returns.

Table 3.7 reports estimates of the policy effect on the treatment firms from nearest neighbor matching. The outcome variable is buy-and-hold return since the beginning of month 0. The policy effect is estimated at two time points: the end of month 6 (results reported in the top panel) and the end of month 12 (results reported in the bottom panel). In the baseline specification in Column (1), each treatment firm is matched to three control firms from the same industry. The matching variables used to determine “distance” are Market Value by the end of month -1, Book-to-Market Ratio by the end of month -4, and Momentum, i.e., the stock return from month -12 to month -1. These matching variables have been shown to be predictors of the future stock return and have been widely used in event studies (Carhart, 1997; Lee, 1997; Wruck and Wu, 2009; He, Yang and Guan, 2010). The distance between any pair of firms is calculated under the Mahalanobis metric. The ATET estimates are adjusted for large sample bias when more than one continuous matching variables are used according to Abadie and Imbens (2006) and Abadie and Imbens (2011).

The point estimate in the top panel of Column (1) suggests that the policy change lowered a treatment firm’s stock return by 8.64 percentage point between month 0 and month 6. This estimate is robust to varying numbers of matching neighbors (Column 2-3) or varying combinations of matching variables (Column 4-5). The estimates of the policy effect at the end of month 12 are mostly close to those at the end of month 6.

Figure 3.2 takes more snapshots over time to show how the policy effect developed. It plots the treatment effect on buy-and-hold returns by the end of each month after the policy change up to month 18. The pattern in Figure 3.2 affirms what we learned from the DID regressions: the treatment effect accumulated during the first 6 to 7 months after the policy change and then stabilized for another 6 months.

The point estimates from matching methods are systematically lower in absolute value than those from the DID approach. This may arise for several reasons. First, DID regressions control firm characteristics only parametrically, while the matching method allows any non-parametric relationships between control variables and stock market returns. There are some firms from the control group that are included in the DID regressions but are not matched to any treatment firms in the matching estimation. If these unmatched control firms enjoyed higher stock returns than the matched control firms in the post-treatment period, the point estimate from a DID regression

Table 3.7: Nearest Neighbor Matching Estimations

Dependent Variable	(1)	(2)	(3)	(4)	(5)
	6-Month Buy-and-Hold Return				
ATET	-8.64*** (2.15)	-8.13*** (2.89)	-8.82*** (2.05)	-7.02*** (2.01)	-7.64*** (2.00)
# of treatment firms	249	249	245	250	250
# of control firms matched	515	210	691	514	505
Total # of control firms	1,027	1,027	1,017	1,036	1,028
Dependent Variable	12-Month Buy-and-Hold Return				
ATET	-8.70*** (3.21)	-5.75 (3.99)	-8.76*** (3.07)	-8.18** (3.30)	-9.45*** (3.15)
# of treatment firms	236	236	232	237	237
# of control firms matched	487	199	648	475	487
Total # of control firms	1,007	1,007	996	1,018	1,010
# of match(es) per treatment firm	3	1	5	3	3
Matching Variables					
Industry	X	X	X	X	
Market Value	X	X	X	X	X
Book-to-Market Ratio	X	X	X		X
12-month Stock Return	X	X	X		X

**Notes:** Standard errors are robust Abadie-Imbens standard errors with 2 matches. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Estimators in Column (1)-(3) and Column (5) are bias-adjusted, following Abadie and Imbens (2006) and Abadie and Imbens (2011). The bias-adjusted covariates are the same as the matching covariates in that column. Market Value and Past 12-month Return are measured by the end of month -1 (September 2013). Book-to-Market Ratio is measured by the end of month -4 (June 2013). Firms from an entire industry will be dropped from analysis if the industry includes too few firms. In Column (1)-(4), firms in telecommunication industry are dropped. Column (3) further drops firms in the public utility industry.

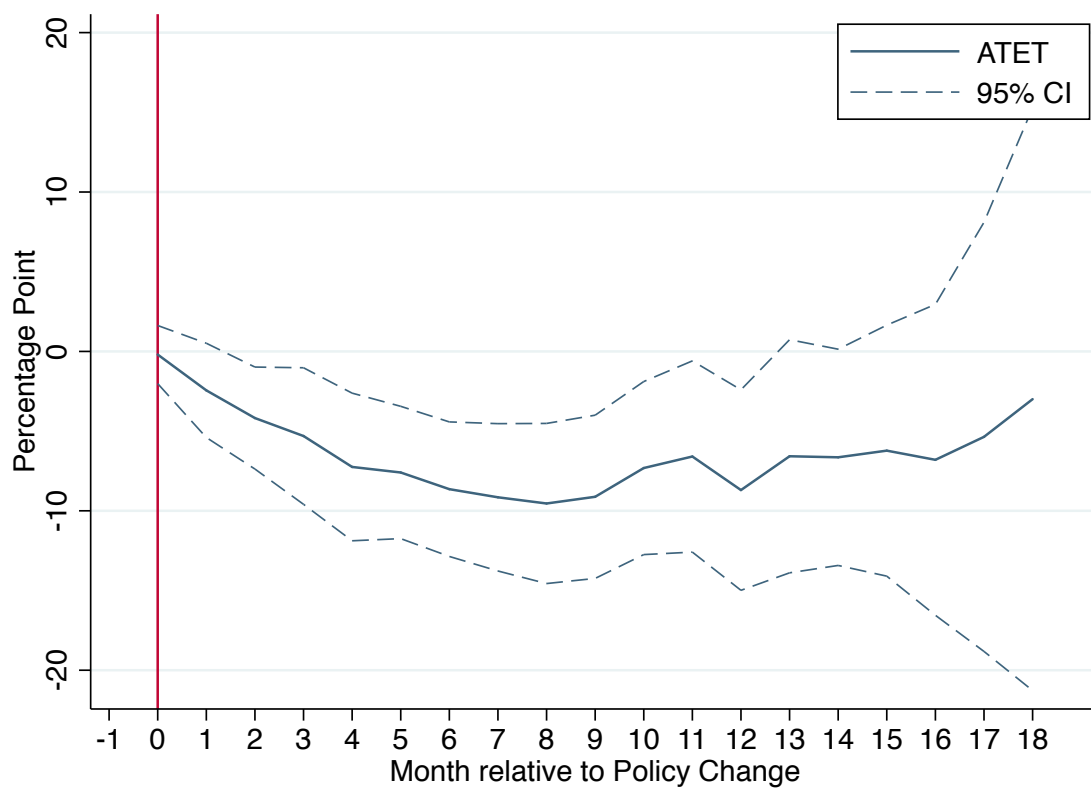


Figure 3.2: ATET from Matching Estimations over Time

**Notes:** ATET estimates are from nearest neighbor matching. The matching algorithm is the same as that in Table 3.7 Column (1).

will be biased away from zero. Second, the control variables used in the DID regressions are measured 12 months before the policy change, while those used in matching are measured more immediately before the policy change. The earlier the variables are measured, the less prediction power they have for the post-treatment stock returns. For these reasons, we prefer the matching estimates to the DID estimates.

### **3.4.3 Interpreting the Policy Effect**

The matching estimates have a straightforward real-life interpretation. Think about two investors, A and B. A invested 100 dollars in the stock of a treatment firm immediately before the policy change. At the same time, B invested 100 dollars in the stock of a control firm that looked very similar to the firm in which A invested. A and B both held their stocks for six months before trading them out. The matching estimates above suggest that A would gain 8 to 9 dollars less than B in the investment.

To trace the source of the return discrepancy, a further investigation of the policy change is required. Before the policy change, firms chose their board members to maximize benefits. It turned out that some firms chose to include government officials on the boards, while others did not. In either case, the pre-treatment board structure should reflect the firm's optimal board member choice. The new policy added new restrictions to board member choices. The firms that previously chose to include government officials on the board had to turn to their next-best choice after the policy change. Thus, the policy effect can be interpreted as the value added by the government officials to a firm that had chosen to invite government officials to sit on the board without the new restrictions. The identifying assumption is that, in equilibrium, firms having chosen their optimal board structures before the policy change have the same stock market return conditional on their observed characteristics.

### **3.4.4 A Placebo Test**

The previous analyses have identified a discrepancy between the stock returns of the treatment firms and the control firms after the policy change. Before we can attribute this discrepancy to the removal of government officials, alternative explanations need to be ruled out. Researchers may concern that it is the political environment created by the anti-corruption campaign, but not the specific policy shock on government-official outside directors, that have caused the treatment firms and the control firms to perform differently. For example, Lin et al. (2016) have documented

in an event study that the effect of the anti-corruption campaign varies among private firms.

This section addresses this concern by conducting a placebo test. We assume a placebo treatment at month -12, redo the matching analyses and calculate placebo policy effects since month -12. The placebo policy effect should be zero before the actual policy change, if the discrepancy between the stock returns of the treatment firms and the control firms is caused by the new policy.

Table 3.8 reports the estimates of the placebo effect. It replicates Table 3.7 while replacing the actual treatment at month 0 with the placebo treatment at month -12. The matching variables are also pushed back by 12 months. Throughout Table 3.8, the matching variables used to determine “distance” are market value by the end of month -13, book-to-market ratio by the end of month -16, and stock return from month -24 to month -13. None of the placebo ATET estimates are significantly different from zero, which suggests that the stock returns of treatment firms and control firms did not diverge before the policy change actually happened.

Figure 3.3 takes more snapshots over time to show how the placebo treatment effect developed from month -12 to month 12. We find that the estimated placebo ATET is close to zero before month 0, when no treatment has actually taken place. The “placebo ATET” estimate becomes more and more negative only after month 0, i.e. when the true policy change happened. The segment between month 0 and month 12 in Figure 3.3 resembles the segment of the same time period in Figure 3.2. This affirms the pattern that the treatment effect arose and accumulated during the first 7 months after the actual policy change and then remained for some time. Note that the two segments are not exactly the same because the matching variables used in the two figures are measured at different time points.

### 3.5 Treatment Heterogeneity

In the previous section, we have established evidence that the removal of government officials from the board lead to lower stock returns of the affected firms. The ATET on the stock return is 8 to 9 percentage points. As the first step to explore potential treatment heterogeneity, Figure 3.4 depicts the distribution of the implied treatment effect of all treated firms. The implied firm-specific treatment effect equals to the buy-and-hold return of a treatment firm less the mean of buy-and-hold return of three matched control firms. The matching algorithm is the same as that

Table 3.8: Nearest Neighbor Matching Estimations: A Placebo Test

Dependent Variable	(1)	(2)	(3)	(4)	(5)
	6-Month Buy-and-Hold Return				
Placebo ATET	2.44 (1.99)	0.46 (2.39)	1.51 (1.98)	1.43 (2.10)	1.38 (2.09)
# of treatment firms	247	247	243	254	248
# of control firms matched	209	498	669	506	518
Total # of control firms	1,023	1,023	1,013	1,071	1,026
Dependent Variable	12-Month Buy-and-Hold Return				
Placebo ATET	2.90 (3.95)	-2.29 (4.88)	4.20 (3.80)	4.15 (3.82)	1.76 (4.00)
# of treatment firms	248	248	244	255	249
# of control firms matched	208	491	656	503	518
Total # of control firms	1,008	1,008	998	1,056	1,011
# of match(es) per treatment firm	3	1	5	3	3
Matching Variables					
Industry	X	X	X	X	
Market Value	X	X	X	X	X
Book-to-Market Ratio	X	X	X		X
12-month Stock Return	X	X	X		

**Notes:** Standard errors are robust Abadie-Imbens standard errors with 2 matches. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Estimators in Column (1)-(3) and Column (5) are bias-adjusted, following Abadie and Imbens (2006) and Abadie and Imbens (2011). The bias-adjusted covariates are the same as the matching covariates in that column. Market Value and 12-month Return are measured by the end of month -13 (September 2012). Book-to-Market Ratio is measured by the end of month -16 (June 2012). Firms from an entire industry will be dropped from analysis if the industry includes too few firms. In Column (1)- (4), firms in telecommunication industry are dropped. Column (3) further drops firms in public utility industry.



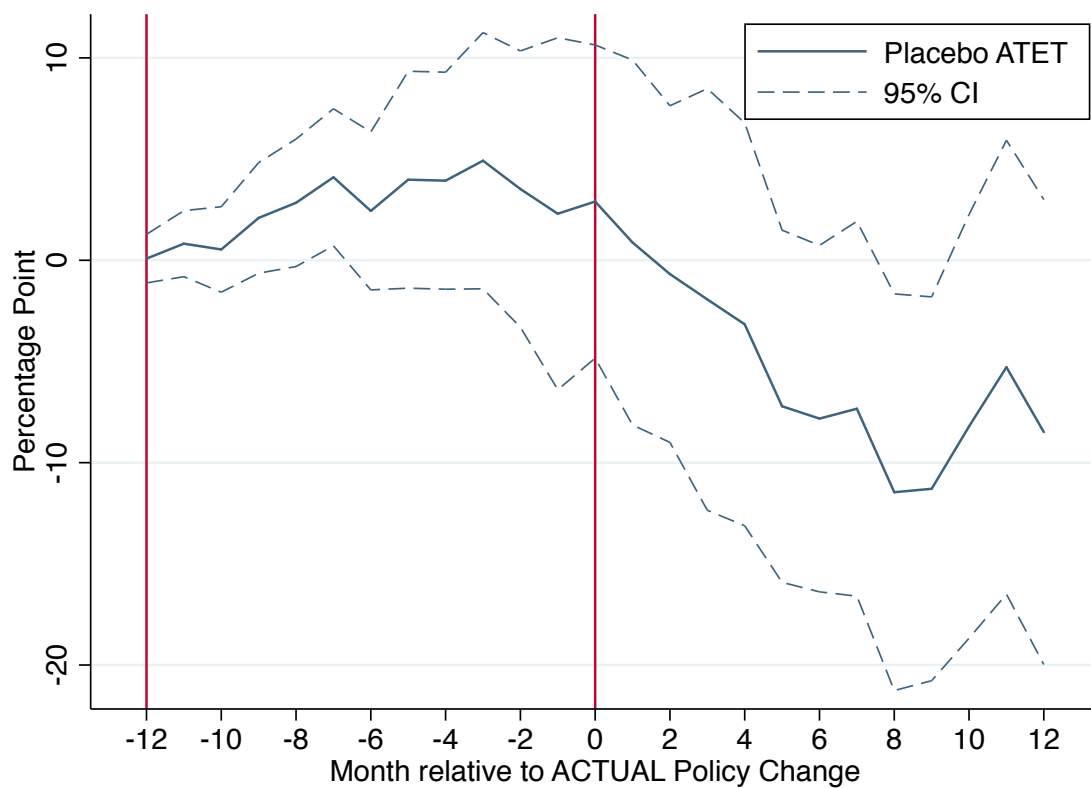


Figure 3.3: Placebo ATET from Matching Estimations over Time

**Notes:** ATET estimates are from nearest neighbor matching. The matching algorithm is the same as that in Table 3.8 Column (1).

in Table 3.7 Column (1). Figure 3.4 shows that the implied policy effect has large variations across treatment firms. The variation could be partly due to stochastic elements. In this section, we explore some factors that have potentially driven the observed treatment heterogeneity.

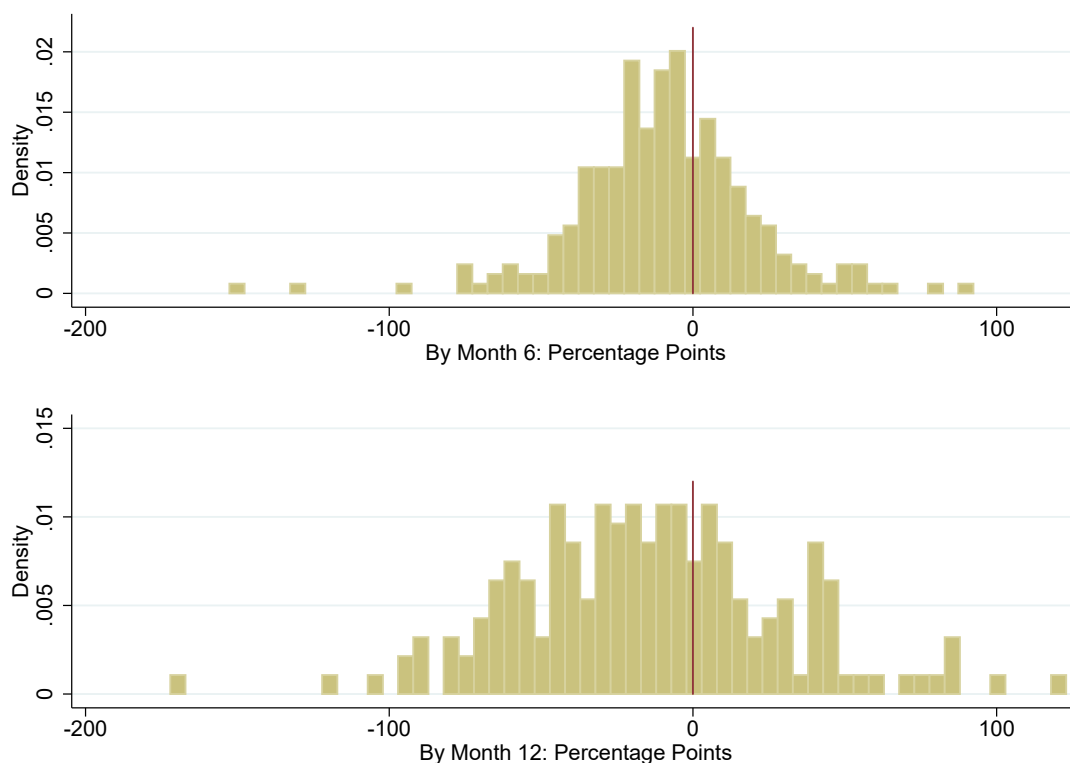


Figure 3.4: Histogram: Estimated Treatment Effect across Firms

### 3.5.1 Government-Official Characteristics

Why do government officials make such different contributions to different firms? One natural explanation is that the government officials themselves have heterogeneous “qualities”. They vary in personal characteristics, work experience, administrative rank, and so on. These factors may determine how much benefit a government official can bring to a firm. When the new policy forced all government officials to leave the boards, it meant different things for different firms: some lost very useful board members, and thus were badly affected, others lost only mediocre ones, and thus were affected less.

We first test this explanation in a difference-in-difference framework. A triple-interaction term is added to Equation (3.1). The following equations are estimated:

$$Y_{it} = \alpha + \beta(T_i \times P_t) + \chi^k(T_i \times P_t \times G_i^k) + \delta_1 T_i + \delta_2 G_i^k + \rho P_t + \gamma \mathbf{X}_i + \epsilon_{it}, \forall k. \quad (3.3)$$

As in Equation (3.1), the dependent variable  $Y_{it}$  is firm  $i$ 's stock return in a 12-month-long or 6-month-long period  $t$ .  $T_i$  is the treatment group indicator and  $P_t$  is the post-treatment period indicator. The coefficient on the interaction between  $T_i$  and  $P_t$ ,  $\beta$ , is the regular treatment effect from a DID regression.  $\mathbf{X}_i$  is a vector of predetermined controls. Time-fixed effects are not included because only two time periods are used in the estimations in this section.

$G_i^k$  is a binary variable that describes a certain characteristic,  $k$ , of the government-official working in firm  $i$ . Each  $G_i^k$  divides the treatment group into two subgroups. If the treatment effect is correlated with government official characteristic  $k$ , then the coefficient on the triple-interaction term,  $\chi^k$ , should be significantly different from zero.

Table 3.9 reports the estimation results of Equations 3.3. The top panel uses 6-month return as the dependent variable and the bottom paned uses 12-month return. All regressions include the same predetermined controls as specified in the table notes. The benchmark regressions are reported in Column (1). They replicate results in Table 3.6 Column (4) and Column (2). In what follows, six characteristics derived from the government officials' profiles are investigated separately: High-Rank, Experienced, Leave-Firm-Late, Hold-Current-Position, Worked-in-Local-Government, Worked-in-Central-Government.

Table 3.9: Difference-in Difference Regressions with Government-Official Characteristics

Official's Characteristics Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		High Rank	Experi- enced	Leave Firm Late	Hold Current Positions	from Local Gov.	from Central Gov.
Treat $\times$ After	-10.03*** (2.66)	-8.70** (3.63)	-1.66 (4.33)	-8.31 (6.75)	-13.74** (4.89)	-11.33 (9.09)	-9.76** (3.34)
Treat $\times$ After $\times$ Official's Characteristic		-2.77 (7.19)	-15.13 (9.15)	-3.01 (8.61)	9.15 (9.76)	1.89 (10.80)	-0.72 (10.45)
Outcome Mean	22.17	22.17	22.17	22.17	22.17	22.17	22.17
Observations	2,490	2,490	2,490	2,490	2,490	2,490	2,490
R-squared	0.15	0.15	0.15	0.15	0.15	0.15	0.15
Dependent Variable	12-Month Stock Return						
Treat $\times$ After	-12.62** (4.29)	-5.11 (6.62)	-6.01 (4.53)	-9.38 (8.75)	-13.91** (5.57)	-16.55** (7.17)	-8.86* (4.27)
Treat $\times$ After $\times$ Official's Characteristic		-15.53 (9.59)	-12.05 (9.66)	-5.71 (10.59)	3.19 (8.58)	5.76 (7.75)	-9.92 (6.07)
Outcome Mean	45.82	45.82	45.82	45.82	45.82	45.82	45.82
Observations	2,512	2,512	2,512	2,512	2,512	2,512	2,512
R-squared	0.13	0.13	0.13	0.13	0.13	0.13	0.13

**Notes:** Standard errors in parentheses. Standard errors are clustered at the industry level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Four control variables are included in each regression. See Table 3.6 for their construction details.

Table 3.9 Column (2) compares firms with high-rank government officials with firms with only low-rank government officials. The variable *High-Rank* takes value 1 if and only if the firm had at least one government official on its board whose administrative rank is higher or equal to *zhengting* (comparable to the head of a municipality). Column (3) explores whether a government official's experience as a board member matters. The variable *Experienced* takes value 1 if and only if the firm had a government official who had been sitting on the board for no less than three years (the length of a typical appointment). Column (4) explores the potential selection in the serving time after the policy change. Recall that while some government official left firms immediately after the new policy was announced, others stay for some time to avoid disruption of firms' management. *Leave-Firm-Late* takes value 1 if and only if the firm had a government official that stayed on the board for more than 12 months after the new policy was announced. Column (5) explores the potential effect of having a *Hold-Current-Position* government official. *Hold-Current-Position* takes value 1 if and only if a firm had a government-official outside director who was holding social positions. Column (6) compares firms with and without local government officials. Column (7) compares firms with and without central government officials.

Due to the small sample size, we do not have enough power to relate a government official's characteristics to their values for a firm: Throughout Table 3.9, none of the coefficients on the triple-interaction terms are statistically significant. The point estimates, however, do have some meaningful suggestions. For example, Column (3) seems to suggest that firms were hurt more severely when they lost an experienced government-official outside directors than when they lost a newer one. This pattern is in line with the intuition that longer-lasting connections are more valuable.

Matching methods can also help to disentangle treatment heterogeneity. To study the correlations between the treatment effect and government-official characteristics, we divide the treatment group into two sub-groups by each  $G^k$ . The ATET on each sub-treatment-group is estimated separately. Table 3.10 reports the ATET on each sub-treatment-group. The dependent variable is buy-and-hold return between month 0 and month 6 in the top panel, and buy-and-hold return between month 0 and month 12 in the bottom panel.

The following example explains how the ATET on each sub-treatment-group is obtained. When estimating the ATET on the *High-Rank* = 0 sub-treatment-group, treatment firms with *High-Rank* = 1 are dropped but all control firms are retained.

Table 3.10a: Nearest Neighbor Matching Estimations on Sub-Groups: *part 1*

	(1)	(2)	(3)	(4)	(5)	(6)
Official's Characteristics	High Rank		Experienced		Leave Firm	Late
Dependent Variable	=0	=1	=0	=1	=0	=1
	12-Month Buy-and-Hold Return					
ATET	-10.1*** (2.83)	-5.30** (2.40)	-6.22* (3.34)	-9.11*** (2.12)	-7.23*** (2.44)	-8.13*** (2.72)
# of treatment firms	131	119	116	134	107	143
# of control firms matched	310	300	291	321	275	344
Total # of control firms	1028	1028	1028	1028	1028	1028
p-value from permutation test	0.16	0.16	0.4	0.42	0.74	0.85
	6-Month Buy-and-Hold Return					
ATET	-9.82** (4.52)	-6.81* (3.67)	-11.6** (4.59)	-5.86 (3.79)	-4.87 (4.14)	-11.0*** (4.08)
# of treatment firms	125	112	110	127	101	136
# of control firms matched	300	280	275	307	252	327
Total # of control firms	1010	1010	1010	1010	1010	1010
p-value from permutation test	0.67	0.52	0.32	0.29	0.23	0.38

Table 3.10b: Nearest Neighbor Matching Estimations on Sub-Groups: *part 2*

Official's Characteristics	(7) Hold Current Position =0	(8) =1	(9) from Local Gov. =0	(10) =1	(11) from Central Gov. =0	(12) =1
Dependent Variable	12-Month Buy-and-Hold Return					
ATET	-7.05*** (2.52)	-8.53*** (2.85)	-5.26 (3.30)	-8.97*** (2.36)	-8.66*** (2.48)	-6.35** (3.03)
# of treatment firms	153	97	80	170	155	95
# of control firms used in match	367	251	211	387	362	244
Total # of control firms	1028	1028	1028	1028	1028	1028
p-value from permutation test	0.58	0.76	0.3	0.3	0.53	0.52
Dependent Variable	6-Month Buy-and-Hold Return					
ATET	-6.41 (4.03)	-11.2*** (4.23)	-7.24 (5.36)	-9.85*** (3.60)	-10.2*** (3.84)	-6.66 (4.77)
# of treatment firms	141	96	76	161	147	90
# of control firms used in match	338	238	198	378	350	228
Total # of control firms	1010	1010	1010	1010	1010	1010
p-value from permutation test	0.3	0.46	0.71	0.62	0.55	0.56

**Notes:** Standard errors are robust Abadie-Imbens standard errors with 2 matches. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Each treatment firm is matched to three control firms. The matching algorithm is the same as that in Table 3.7 Column (5). The null hypothesis of the permutation test is " $\mathbf{H}_0$ : the ATET of the sub-treatment-group = the ATET of the treatment group." The tests are conducted by permuting the subgroup assignment among the treatment firms.

Each treatment firm with *High-Rank* = 0 is matched to three control firms based on *Market Value*, *Book-to-Market Ratio*, and *Momentum* as those used in Table 3.7. Due to insufficient observations in each sub-treatment-group, matching on industry is not required. The matching algorithm employed here is thus the same as that in Table 3.7 Column (5).

The estimated ATET in Table 3.10 does not vary substantially across subgroups. This observation can be justified by permutation tests. Along with each ATET estimate, the  $p$ -value of the permutation test is reported. The null hypothesis of the test is

$\mathbf{H}_0$ : *the ATET of the sub-treatment-group = the ATET of the treatment group.*

In none of the subgroups  $\mathbf{H}_0$  is rejected, which suggests that these observed government-official characteristics cannot explain the treatment heterogeneity.

We are not able to connect the heterogeneity in treatment effect to any of the government-official characteristics discussed above. But this does not invalidate the hypothesis that a government official's value-added to a firm depends on his or her "quality". The "quality", however, cannot be easily measured by observables. Some unobserved government-official characteristics may be of great relevance, for example, a government official's actual influencing power and work efforts. Without further information, we cannot fully answer the question that "what kind of government officials bring more value for a firm".

### 3.5.2 Firm Characteristics

To explain treatment heterogeneity, we turn to another dimension: firm characteristics. Political connections through an outside director may have different values for firms of different sizes, in different stages of development or under different operating conditions. What kinds of firms enjoy more benefits from having government officials on the board? We estimate a DID equation with a triple-interaction term that is similar to Equation (3.3):

$$Y_{it} = \alpha + \beta(T_i \times P_t) + \chi^l(T_i \times P_t \times F_i^l) + \nu_1(T_i \times F_i^l) + \nu_2(T_i \times F_i^l) + \delta_1 T_i + \delta_2 F_i^l + \rho P_t + \gamma \mathbf{X}_i + \epsilon_{it}, \forall l. \quad (3.4)$$



$F_i^l$  is a binary variable that describes the characteristic  $l$  of firm  $i$ . Each  $F_i^l$  divides the all sample firms into two subgroups. If the treatment effect is correlated with the firm characteristic  $l$ , then the coefficient on the triple-interaction term,  $\chi^l$ , should be significantly different from zero. Other notations are the same as those in Equations 3.3.

We studied five different  $F_i^l$ 's in this section. Table 3.11 reports the results. The top panel uses 6-month return as the dependent variable and the bottom paned uses 12-month return. All regressions include the same predetermined controls. Column (1) is the benchmark DID regression replicating Table 3.6 Column (4) and Column (2). In Column (2)-(6), the treatment indicator interacts with five firm characteristics one by one. The characteristic variables are defined in the table notes. None of the triple-interaction terms are statistically significant.

Table 3.11: Difference-in-Difference Regressions with Firm Characteristics

Firm Characteristics Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)
		Big	New 6-Month Stock Return	Developed Province	High Q Industry	Concentrated Structure
Treat $\times$ After	-10.03*** (2.66)	-8.37 (5.11)	-8.02* (4.07)	-9.81* (4.74)	-10.45*** (2.83)	-11.26 (6.65)
Treat $\times$ After $\times$ Firm Characteristic		-1.47 (6.37)	-4.12 (10.12)	-0.74 (8.00)	1.65 (5.83)	2.53 (10.82)
Outcome Mean	22.17	22.17	22.17	22.17	22.17	22.17
Observations	2,490	2,490	2,490	2,490	2,490	2,490
R-squared	0.15	0.15	0.15	0.15	0.15	0.15
Dependent Variable				12-Month Stock Return		
Treat $\times$ After	-12.62** (4.29)	-13.52* (6.41)	-6.25 (5.14)	-11.50 (7.31)	-13.51** (5.07)	-9.70 (6.38)
Treat $\times$ After $\times$ Firm Characteristic		4.27 (6.86)	-12.94 (9.10)	-2.34 (7.91)	2.80 (8.29)	-5.97 (9.00)
Outcome Mean	45.82	45.82	45.82	45.82	45.82	45.82
Observations	2,512	2,512	2,512	2,512	2,512	2,512
R-squared	0.13	0.13	0.13	0.13	0.13	0.13

**Notes:** Standard errors in parentheses. Standard errors are clustered at the industry level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Big indicates the above-median registered capital (of all sample firms). New indicates below-median firm age. Developed Province indicates that a firm is registered in provinces with a high marketization level according to the Marketization Index by (Fan, Wang and Zhu, 2011). High-Q-Industry indicates that a firm is in an industry that has a high Q-value in general. Concentrated-Structure indicates that the top 10 shareholders' share combined being greater than 61.5%. Four control variables are included in each regression. See Table 3.6 for their construction details.

There is no straightforward way to relate the treatment heterogeneity to either observed government-official characteristics or firm characteristics. The value of a government-official as a board member for a firm seems to be driven by unobserved factors. As discussed above, we do not have good measures of a government official's influencing power or work efforts, which are potentially very important factors. Some hidden firm characteristics may also be relevant. For example, some firms have indirect business relationships with the government, other firms may have hidden political connections that can substitute government officials on the board. Moreover, it could be the match between a government official and a firm that matters. Future research can revisit this question when more detailed data are collected.

### 3.6 Policy Effects on the Real Side

Empirical evidence in the previous sections suggests that government officials on the board can affect the firms' stock prices. This section goes one step further to explore how government officials can influence the firms' real business performance. A large range of indexes derived from annual reports are used to measure firms' performance from different perspectives. The following equation is estimated.

$$Y_{it} = \alpha + \beta(T_i \times \mathbb{I}_{(t=2014)}) + \delta T_i + \rho \mathbb{I}_{(t=2014)} + \epsilon_{it} \quad (3.5)$$

The dependent variable in Equation (3.5) is a performance index.  $T_i$  is the treatment indicator as that in Equation (3.1). The treatment effect is captured by the coefficient on the interaction term,  $\beta$ .

Annual reports from two years, 2012 and 2014, are used to include a full pre-treatment year and a full post-treatment year. Data from the transition year, 2013 are excluded. The reason we restrict the analyses to only two years is to minimize endogenous turnovers of non-government-official board members.

Table 3.12 reports the estimation results. In each regression in Table 3.12, only firms with non-missing dependent variables for both 2012 and 2014 are used. The first two columns measure the treatment effect on business expansion and overall profitability, respectively. The dependent variable in Column (1) is the logarithm of operating revenue. The (insignificant) point estimate suggests that operating revenue for the treatment group dropped by 4 % in the post-treatment year, 2014. The treatment effect on profitability is more prominent, as suggested by Column (2). The

probability of acquiring positive profit for the treatment firms dropped by 0.05 because of the policy change. This result is consistent with the drop in stock return.

Table 3.12a: Policy Effects on the Real Side: *part 1*

Variables	(1) Log Op- erating Revenue	(2) Positive Profit	(3) ROIC	(4) Gross Margin	(5) Tax to Profit	(6) Cash Inflow
Treat×Year2014	-0.04 (0.05)	-0.05*** (0.01)	-0.27 (1.29)	0.99 (2.42)	-2.42 (2.84)	1.37 (2.45)
Treat	0.27*** (0.06)	-0.01 (0.02)	-1.01 (1.11)	-1.29 (0.75)	-0.24 (1.39)	-1.16 (1.53)
Year2014	0.28*** (0.03)	-0.02** (0.01)	-1.64 (1.10)	-2.30 (2.70)	2.27 (2.30)	-0.96 (2.27)
Constant	20.57*** (0.14)	0.86*** (0.01)	7.29*** (0.85)	30.45*** (2.99)	17.45*** (1.13)	100.13*** (1.86)
Outcome Mean	20.75	0.840	6.256	29.15	18.31	99.56
Observations	2,720	2,724	2,724	2,716	2,328	2,716
R-squared	0.02	0.00	0.00	0.00	0.00	0.00

Aiming to explore the mechanisms of the treatment effect on firms' profitability, Column (3)-(12) analyze more business performance measures: profit quality (Column 3-4), tax burden (Column 5), cash adequacy (Column 6-7), asset structure (Column 8), turnover (Column 9-10), R & D investment (Column 11), and subsidy receipt (Column 12). Definitions of dependent variables are in table notes. No significant results are found with these measures.

In summary, the removal of political connections through outside directors affects firms' overall profitability. There are very limited findings, however, on the mechanisms. Several reasons might explain why. First, the indexes we used here are not as sensitive as the stock price and are measured only once a year. Many confounding factors may interfere with firm performance between two measuring time points and thus contaminate the policy effect. This makes the policy effect, even if it exists, hard to detect. Second, when discussing treatment heterogeneity, we have shown that the estimated value of political connection varies dramatically across firms. The working mechanisms of political connection may also vary across firms. Government officials may help different firms in different aspects. As a result, when averaging each aspect of performance across all firms, we cannot find a significant effect of political connection on any one of the aspects.

Table 3.12b: Policy Effects on the Real Side: *part 2*

	(7)	(8)	(9)	(10)	(11)	(12)
Variables	Cash to Invest	Asset to Equity	Business Turnover	Full Asset Turnover	Positive R&D	Received Subsi- dies
Treat $\times$ Year2014	0.14 (0.67)	-0.08 (0.05)	182.41 (214.05)	-0.01 (0.03)	0.01 (0.02)	-0.02 (0.01)
Treat	-0.18 (0.54)	0.15 (0.11)	9.49 (122.17)	0.02 (0.04)	-0.03 (0.03)	0.03** (0.01)
Year2014	0.51 (0.28)	0.11*** (0.03)	9.91 (82.59)	-0.02 (0.02)	0.01 (0.01)	0.08*** (0.01)
Constant	0.08 (0.24)	1.85*** (0.09)	374.75*** (87.16)	0.66*** (0.05)	0.85*** (0.04)	0.85*** (0.03)
Mean Outcome	0.316	1.926	398.9	0.650	0.850	0.896
Observations	2,558	2,686	2,712	2,722	2,724	2,724
R-squared	0.00	0.00	0.00	0.00	0.00	0.02

**Notes:** Standard errors in parentheses. Standard errors are clustered at the industry level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Operating Revenue is measured in million RMB (Chinese *yuan*). Positive Profit indicates the adjusted profit being positive. ROIC is short for Returns on Invested Capital. The unit of ROIC, Gross Margin, Tax to Profit and Cash Inflow is a percentage point. The Business Turnover is measure in days. The Full Asset Turnover is the the ratio of totoa revenue to total assets. Positive R& D indicates the R&D expenditure being positive. Received Subsidies indicates that the firm received government subsidies. Four control variables are included in each regression. See Table 3.6 for their construction details.

### 3.7 Conclusions

In this paper, we examine a policy that removed government officials from the boards to estimate the value of this type of political connections for firms. The empirical analyses suggest that removing government officials from the board makes an affected firm yield 8 percentage point less return in the stock market in the next 12 months. The policy change exogenously cut firms' political connections through outside directors, but left other aspects of the firms untouched. The observed policy effect can thus be interpreted as the value of this type of political connections.

The policy effect varies across firms, which suggests that government officials on the board have different values for different firms. The variation cannot be explained well by either observed government-official characteristics or firm characteristics. Future research can revisit the heterogeneity in values when more detailed data are available.

We also studied the policy effects on a large range of real business performance measures of firms. Although, given our limited data on business performance, we cannot draw conclusions on the mechanisms of how political connections help firms, we do find that the affected firms, on average, became less profitable due to the loss of political connections. This finding is consistent with the findings in the stock return changes. The mechanisms suggested by previous studies could apply in this paper's context. The value of government-officials as outside directors could take different forms. They could bring firms preferential government treatment, relaxed regulatory oversight, business opportunities in the public sector or insider's information on public policy. In a well-functioning stock market, stock prices reflect the composite benefit from all aspects.

## APPENDICES

## APPENDIX A

### Characteristics of All Survey Respondents

This section reports the characteristics of all respondents of the follow-up survey in Chapter I and that of the coupon-eligible sample.



Table A.1: Characteristics of All Survey Respondents

Variables	Obs.	Full Survey Sample Mean (s.d.)	Coupon- Eligible Sample Mean (s.d.)	Diff: tested neg. wi 3m minus coupon- eligible (p-value)	Diff: tested pos. minus coupon- eligible (p-value)
Indicator: female	2,551	0.700 (0.458)	0.673 (0.469)	0.029 (0.228)	0.119*** (0.000)
Age	2,530	36.074 (14.807)	36.259 (15.571)	-2.323*** (0.003)	1.434* (0.079)
Education in years	2,523	6.146 (3.981)	5.907 (4.009)	0.967*** (0.000)	0.033 (0.885)
Indicator: respondent provided a phone number	2,551	0.525 (0.499)	0.509 (0.500)	0.043* (0.070)	0.086*** (0.003)
# of sex partners in the last 12 months: none	2,330	0.162 (0.369)	0.174 (0.379)	-0.077*** (0.000)	0.006 (0.820)
# of sex partners in the last 12 months: only one	2,330	0.725 (0.446)	0.716 (0.451)	0.064*** (0.008)	-0.003 (0.923)
# of sex partners in the last 12 months: more than one	2,330	0.112 (0.316)	0.110 (0.313)	0.013 (0.447)	-0.003 (0.886)
HIV-test history: never tested	2,519	0.268 (0.443)	0.431 (0.495)	-0.388 (0.000)	-0.442*** (0.000)
HIV-test history: tested more than one year ago	2,519	0.242 (0.428)	0.254 (0.436)	-0.265*** (0.000)	0.347*** (0.000)
HIV-test history: tested within one year	2,519	0.490 (0.500)	0.315 (0.465)	0.653*** (0.000)	0.095*** (0.002)
# of correct answers out of 15 HIV questions	2,367	11.932 (3.408)	11.572 (3.664)	0.730*** (0.000)	1.101*** (0.000)
Subject risk of HIV+: the higher the riskier	2,138	1.746 (0.942)	1.886 (0.982)	-0.408*** (0.000)	0.000
Distance (km) between the household and a clinic	2,471	2.221 (2.832)	2.348 (3.135)	-0.007 (0.743)	0.007 (0.775)
Indicator: household go without food sometimes	2,550	0.576 (0.494)	0.604 (0.489)	-0.040* (0.091)	0.074*** (0.004)
Indicator: household has HIV+ member	2,392	0.228 (0.420)	0.090 (0.286)	0.010 (0.528)	0.888*** (0.000)
1st principal component of 14 assets†	2,551	0.714 (2.018)	0.648 (2.036)	0.239** (0.015)	-0.216** (0.046)

**Notes:** The p-values are from t-tests of equality. The t-tests are controlled for community fixed-effects and enumerator fixed-effects.

† The 14 assets are the same as those in Table 1.2

Table A.2: Characteristics of the Coupon-Eligible Sample

Variables	Obs.	Coupon-Eligible Sample Mean (s.d.)	Concerned Sample Mean (s.d.)	Diff: Un-concerned minus Concerned (p-value)
Indicator: female	1,588	0.673 (0.469)	0.675 (0.469)	-0.015 (0.601)
Age	1,575	36.259 (15.571)	36.334 (15.838)	0.551 (0.591)
Education in years	1,574	5.907 (4.009)	6.012 (4.066)	-0.158 (0.513)
Indicator: respondent provided a private phone number	1,588	0.509 (0.500)	0.525 (0.500)	0.010 (0.725)
# of sex partners in the last 12 months: none	1,453	0.174 (0.379)	0.183 (0.387)	-0.034 (0.190)
# of sex partners in the last 12 months: only one	1,453	0.716 (0.451)	0.694 (0.461)	0.074** (0.015)
# of sex partners in the last 12 months: more than one	1,453	0.110 (0.313)	0.123 (0.329)	-0.040* (0.047)
HIV-test history: never tested	1,570	0.431 (0.495)	0.422 (0.494)	-0.002 (0.938)
HIV-test history: tested more than one year ago	1,570	0.254 (0.436)	0.253 (0.435)	-0.005 (0.842)
HIV-test history: tested within one year	1,570	0.315 (0.465)	0.325 (0.469)	0.008 (0.792)
# of correct answers out of 15 HIV questions	1,480	11.572 (3.664)	11.721 (3.128)	-0.809*** (0.003)
Subject risk of HIV+: the higher the riskier	1,541	1.886 (0.982)	1.836 (0.957)	0.053 (0.345)
Distance in km between the household and a clinic	1,540	2.348 (3.135)	2.231 (3.107)	0.018 (0.508)
Indicator: household go without food sometimes	1,588	0.604 (0.489)	0.592 (0.492)	0.040 (0.136)
Indicator: household has HIV+ member	1,469	0.090 (0.286)	0.080 (0.272)	0.023 (0.231)
1st principal component of the ownership of 14 assets†	1,588	0.648 (2.036)	0.757 (2.087)	0.071 (0.516)

**Notes:** The p-values are from t-tests of equality. The t-tests are controlled for community fixed-effects and enumerator fixed-effects.

† The 14 assets are the same as those in Table 1.2

## APPENDIX B

### Robustness Checks for the Definition of Test Uptake

Test uptake is the primary outcome of interest. In this paper, it is measured by coupon redemption. Specifically, I follow the rules below to code the test uptake indicator:

1. It takes value 1 only if the coupon was redeemed by an adult of the same gender as the coupon recipient.
2. It takes value 1 only if the coupon was redeemed within 14 days as instructed by the enumerators.
3. If a coupon's code was not properly read when distributing,<sup>1</sup> it is considered "not redeemed".
4. If a participant received special coupons,<sup>2</sup> he or she remained in the analysis sample.

This section shows that the conclusions we obtained in the paper are robust to alternations of the rules above. Consider four alternative ways to code test uptake.

Alternative 1: follows rule 2-4, but drops rule 1;

Alternative 2: follows rule 1, 3, 4, but drops rule 2;

Alternative 3: replace rule 3 with

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<sup>1</sup>Due to technical errors, 2.3% of the coupons distributed were not correctly read by scanners, and thus cannot be linked to redeemed coupons.

<sup>2</sup>To incentivize truth-telling in the willingness-to-accept measure, we introduced special coupons (See the Pre-Analysis Plan for details). 9.2% of the participant from the Control Group and the Concern-Relieving Intervention Group ended up received special coupons.

Alt3. If a coupon's code was not properly read when distributing, exclude the coupon recipient from the sample;

Alternative 4: replace rule 4 with

Alt4. If a participant received special coupons, exclude this participant from the sample.

Table B.1 Panel A and Panel B replicate regressions in Table 1.3 and Table 1.4, respectively, with the four alternate definitions of test uptake. The estimated treatment effect is stable across all four alternative definitions.

Table B.1: Robustness to Alternate Definitions of Test Uptake

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Definitions	Original									
<b>Panel A</b>	Alternative 1									
Concern-Relieving Intervention	0.0634** (0.0315)	0.0771** (0.0326)	0.0510 (0.0345)	0.0735** (0.0356)	0.0608* (0.0315)	0.0733** (0.0327)	0.0647** (0.0321)	0.0825** (0.0335)	0.0520 (0.0332)	0.0657* (0.0351)
Observations	754	754	754	754	754	754	733	733	666	666
R-squared	0.006	0.292	0.003	0.290	0.005	0.292	0.006	0.303	0.004	0.304
<b>Panel B</b>										
Concern-Relieving Intervention	0.0634** (0.0315)	0.0686** (0.0323)	0.0510 (0.0345)	0.0611* (0.0352)	0.0608* (0.0315)	0.0647** (0.0324)	0.0647** (0.0321)	0.0719** (0.0332)	0.0520 (0.0332)	0.0594* (0.0348)
High-Incentive	0.119*** (0.0377)	0.120*** (0.0389)	0.130*** (0.0423)	0.140*** (0.0422)	0.125*** (0.0378)	0.126*** (0.0390)	0.116*** (0.0381)	0.120*** (0.0394)	0.116*** (0.0386)	0.107*** (0.0405)
Observations	996	996	996	996	996	996	973	973	906	906
R-squared	0.011	0.247	0.012	0.258	0.012	0.246	0.011	0.252	0.011	0.263
Control Group mean	0.207	0.207	0.255	0.255	0.210	0.210	0.214	0.214	0.213	0.213
Constant	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Controls	no	yes	no	yes	no	yes	no	yes	no	yes

**Notes:** Standard errors in parentheses. Standard errors are clustered at the household level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1  
The control variables are the same as in Table 1.3.

## APPENDIX C

### Analysis Specified in the Pre-Analysis Plan

#### Recruitment and Randomization Procedures

The participants for my experiment were drawn from the “vulnerable” households survey in the baseline between May 2017 and March 2018. A household was considered “vulnerable” if it satisfied at least one of the following 11 criteria at the time of the baseline survey:

1. Some child’s parents were not living in the household;
2. The children-to-adults ratio was greater than 4;
3. Some school-aged child was not at school;
4. The household ate only one meal per day on average;
5. Household went someday without food;
6. Household’s primary source of income was illegal or had no source of income;
7. Some household member was chronically ill;
8. Some household member was HIV+;
9. Some household member was on Anti-Retroviral Therapy (ART);
10. Some child was orphaned;
11. Some adults died of chronic illnesses in the past 5 years.

Before the research team started recruiting for the stigma experiment, each “vulnerable” household was randomly assigned a priority order to be recruited and a group status, control, concern-relieving, and high-incentive. The randomization of

priority order and group status are orthogonal to each other and stratified at the community level. The pre-assigned group status was programmed into the survey software and was concealed from enumerators. Enumerators approached the households according to the priority order until the targeted number of households were recruited. A household’s group assignment was revealed to the participant and the enumerator after the recruitment survey, before the distribution of testing coupons.

The study groups mentioned in the pre-analysis plan refer to the group status assigned to the household before any recruitment attempts. At the time of pre-assigning group status, whether a respondent would overestimate stigma was unknown. The “control group” in the pre-analysis plan, hereafter PAP-Control Group, refers to the households assigned not to receive intervention and to receive coupons of 50 Meticaïs. It is the union of the Control Group and part of the Unconcerned Group 1 in Figure 4. The “Anti-stigma” intervention group, hereafter PAP-Intervention Group, in the pre-analysis plan, refers to the households assigned to receive the concern-relieving intervention conditional on being concerned and to receive coupons of 50 Meticaïs. It is the union of the Concern-Relieving Intervention and the rest of Unconcerned Group 1. The “High-Incentive” group is not included in the pre-analysis plan. It was added to the experiment design after the pre-analysis plan was registered.

## Protocol for the Concern-Relieving Intervention

Details in the intervention procedure can be found in the Pre-Analysis Plan under:

Yu, Hang. 2019. “Anti-Stigma Interventions to Encourage HIV Testing in Vulnerable Households in Mozambique.” AEA RCT Registry. October 18. DOI

## Results from the Primary Specification

The primary regression specification is (Equation 1 in the pre-analysis plan):

$$Y_{ihc} = \alpha + \beta_1^{IS} T_h + \beta_2^{IS} S_i + \beta_3^{IS} (T_h \times S_i) + \delta^I \mathbb{X}_i + \delta^H \mathbb{X}_h + \epsilon_{ihc} \quad (\text{C.1})$$

$Y_{ihc}$  is the outcome of interest for individual  $i$  in household  $h$  of community  $c$ .  $T_h$  is the treatment indicator that takes value 1 if household  $h$  was assign to the PAP-Intervention Group and 0 otherwise.  $\mathbb{X}_i$  and  $\mathbb{X}_h$  are the vectors of control variables at the individual level and the household level. Joining them together produces the

control vector used in the paper,  $\mathbb{X}_i$ .  $\epsilon_{ihc}$  is the error term clustered at the household level.  $S_i$  is the binary indicator of overestimating stigma.

The primary hypothesis is  $\beta_3^{IS} > 0$ . The intervention effect identified in the paper is equivalent to  $\beta_1^{IS} + \beta_3^{IS}$ , which means, among the “concerned” individuals, receiving the intervention raises the testing rate. In theory,  $\beta_1^{IS}$  should be equal to 0 as the unconcerned individuals pre-assigned to PAP-Intervention Group did not receive any differing from those received by the PAP-Control Group.  $\beta_1^{IS}$  will pick up any random imbalance between the two groups of people.

Table C.1 reports results from the primary regression specification.

Table C.1: Primary Regression Specification in PAP	
Outcome	(1) Test Uptake
$\beta_1$	-0.0380 (0.0436)
$\beta_2$	-0.0549 (0.0432)
$\beta_3$	0.121** (0.0528)
PAP-Control Group Mean	0.224
Control Group Mean	0.207
Implied Intervention Effect: $\beta_1 + \beta_3$	0.0834
p-value of test: $\beta_1 + \beta_3 = 0$	0.00821
Observations	1,162
R-squared	0.239
Constant	yes
Controls	yes

**Notes:** Standard errors in parentheses. Standard errors are clustered at the household level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1  
The control variables are the same as in Table 1.3.

## Results from Subgroup Analyses

Table C.2 reports results from subgroup analyses.



Table C.2: Intervention Effects on Test Uptake: Subgroup Analysis in PAP

Subgroup	(1) Male	(2) Female	(3) Low educ	(4) High educ	(5) Poor	(6) Wealthy	(7) Low subrisk	(8) High subrisk
$\beta_1$	-0.145* (0.0840)	-0.0434 (0.0575)	0.0547 (0.0640)	-0.0976 (0.0640)	-0.0198 (0.0668)	-0.0684 (0.0698)	0.00881 (0.0632)	-0.0404 (0.0762)
$\beta_2$	-0.109 (0.0883)	-0.0822 (0.0560)	0.0975 (0.0645)	-0.170** (0.0664)	-0.0347 (0.0638)	-0.124* (0.0694)	-0.0177 (0.0622)	-0.0380 (0.0753)
$\beta_3$	0.235** (0.106)	0.115* (0.0679)	-0.0355 (0.0831)	0.259*** (0.0778)	0.0829 (0.0817)	0.185** (0.0816)	0.0760 (0.0771)	0.0950 (0.0945)
PAP-Control Group Mean	0.197	0.237	0.258	0.197	0.244	0.206	0.223	0.232
Control Group Mean	0.175	0.222	0.279	0.156	0.223	0.194	0.233	0.190
$\beta_1 + \beta_3$	0.0896	0.0716	0.0193	0.162	0.0631	0.117	0.0848	0.0546
p-value of test $\beta_1 + \beta_3 = 0$	0.160	0.0649	0.721	0.000301	0.212	0.0148	0.0771	0.340
p-value of equality test of $\beta_3$	0.232		0.003		0.303		0.855	
Observations	381	781	526	628	559	603	611	516
R-squared	0.418	0.304	0.417	0.339	0.382	0.316	0.303	0.356
Constant	yes	yes	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes	yes	yes

**Notes:** Robust standard errors in parentheses. Standard errors are clustered at the household level. Stars attached to the coefficients reflect unadjusted p-values: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Each pair of the  $\beta_3$  are tested against equality. p-values of equality tests are reported. “Low educ” group has participants with below-median years of education (5 years or less); “High educ” group with above-median years of education (6 years or more). “Poor”/“Wealthy” status is determined by the first principle components of ownership of 14 assets. The cut off value if 0.250. Participants with a low subjective risk of infection (“Low subsrisk”) are those believed themselves to be HIV negative. “High subsrisk” group are participants with a low subjective risk of infection (“Low subsrisk”) are those believed positive. In the regressions with controls, the control variables are the same as those in 1.3, while Column (1) (2) drop female indicator, Column (3) (4) drop education level, Column (5) (6) drop asset indicator, Column (7) (8) drop subject risk index.

## Secondary Outcomes of Interest

There are three secondary outcomes of interest: household-level coupon redemption, willingness to accept (WTA) a testing coupon (as opposed to a visiting coupon), changes in beliefs about stigma. For variable definitions and data collection procedures, please see the Pre-Analysis Plan.

Regression analyses on the secondary outcomes of interest are reported in Table C.3.

Table C.3: Secondary Outcomes of Interest

Outcomes	(1) Household level coupon redemption	(2) WTA for a testing coupon	(3) Changes in the belief about stigma
$\beta_1$	-0.0286 (0.0433)	0.707** (0.300)	-0.844 (1.441)
$\beta_2$	-0.0374 (0.0419)	0.296 (0.305)	5.618*** (1.461)
$\beta_3$	0.0669 (0.0527)	-0.690* (0.380)	0.917 (1.651)
PAP-Control Group Mean	0.319	5.502	0.0400
Control Group Mean	0.314	5.550	1.952
$\beta_1 + \beta_3$	0.0383	0.0166	0.0728
p-value of test $\beta_1 + \beta_3 = 0$	0.196	0.944	0.950
Observations	1,408	1,159	344
R-squared	0.196	0.395	0.663
Constant	yes	yes	yes
Controls	yes	yes	yes

**Notes:** Robust standard errors in parentheses. Standard errors are clustered at the household level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. The household-level coupon redemption is an indicator for at least one of the coupons in a household is used. For the definition of WTA and changes in belief, please see the Pre-Analysis Plan. Control variables for Column (1) are Indicator: the head of the household is female (yes, no); Indicator: there is a child living in this household (yes, no); Indicator: the household has a mobile phone (yes, no); Education of the household head: highest grade completed; Primary respondent's knowledge about HIV (Number of correct answers to the 15 questions testing HIV-related knowledge); The straight-line distance between the household and the testing clinic (in km); Square of the straight-line distance between the household and the testing clinic (in km); Indicator: the household ever go without food in the last 12 months (yes, no); Indicator: there is an HIV positive household member (yes, no); Asset ownership index: the first principal component of 14 asset-ownership indicators; Enumerator fixed-effects; Community fixed-effects. Control variables for Column (2) and Column (3) are the same as those in Table 1.3.

## APPENDIX D

### Baseline Measures of Social Stigma Environment

Table D.1: Baseline Stigma Measures in 76 Study Communities

Comm- unity ID	Q1. Would you buy fresh vegetables from a shopkeeper if you knew that this person had HIV? (Yes/No)		Q2. If a member of your family became sick with AIDS would you be willing to care for them in your own household? (Yes/No)		Q3. In your opinion, if a teacher has HIV but is not sick, should they be allowed to continue teaching at school? (Yes/No)	
	# of re- spondents	Share of “yes” answers	# of re- spondents	Share of “yes” answers	# of re- spondents	Share of “yes” answers
1	91	0.813	93	0.989	87	0.954
2	96	0.875	98	0.990	96	0.938
3	98	0.745	99	0.919	98	0.878
4	95	0.684	96	0.854	92	0.750
5	84	0.726	89	0.843	80	0.762
6	83	0.675	84	0.857	63	0.714
7	93	0.925	93	0.968	91	0.923
8	109	0.936	109	0.991	108	0.963
9	106	0.868	106	0.962	105	0.914
10	106	0.755	104	0.942	101	0.851
11	62	0.871	63	1	48	0.875
12	72	0.792	71	0.986	58	0.897

*Continued on the next page*

Table D.1 – *Continued from the previous page*

13	90	0.889	89	0.955	77	0.857
14	59	0.915	60	0.967	52	0.942
15	56	0.893	55	0.982	56	0.911
16	66	0.818	65	1	57	0.895
17	60	0.933	63	0.968	63	0.937
18	62	0.790	66	0.985	53	0.849
19	67	0.716	68	0.897	69	0.870
20	74	0.811	77	0.961	75	0.947
21	62	0.726	64	0.828	63	0.889
22	72	0.750	74	0.959	73	0.904
23	84	0.881	85	0.953	80	0.938
24	81	0.728	85	0.941	84	0.917
25	62	0.645	64	0.875	62	0.871
26	58	0.724	59	0.932	57	0.860
27	67	0.836	68	0.809	68	0.868
28	74	0.892	75	0.973	73	0.932
29	64	0.797	66	0.894	66	0.879
30	61	0.869	63	1	59	0.966
31	61	0.787	62	0.952	62	0.919
32	70	0.743	72	0.903	71	0.915
33	69	0.812	69	0.971	69	0.884
34	65	0.892	66	0.955	64	0.984
35	76	0.803	79	0.924	77	0.883
36	69	0.928	73	0.945	73	0.932
37	67	0.731	69	0.942	68	0.868
38	78	0.833	78	0.846	78	0.833
39	79	0.722	80	0.913	79	0.861
40	68	0.735	69	0.957	65	0.954
41	35	0.629	34	0.882	34	0.794
42	66	0.773	66	0.803	65	0.815
43	50	0.820	50	0.860	50	0.840
44	38	0.789	38	0.895	38	0.842
45	54	0.722	53	0.792	54	0.833
46	48	0.750	48	0.729	49	0.714
47	53	0.717	54	0.870	53	0.925
48	43	0.698	43	0.814	42	0.857

*Continued on the next page*

Table D.1 – *Continued from the previous page*

49	43	0.884	39	0.974	42	1
50	70	0.971	70	1	70	0.986
51	71	0.845	72	0.972	70	0.971
52	50	0.520	57	0.982	53	0.962
53	62	0.839	73	0.986	56	0.857
54	25	0.440	26	0.923	26	0.808
55	31	0.935	32	1	31	0.903
56	34	0.794	36	0.972	33	0.879
57	46	0.783	48	1	47	0.936
58	34	0.735	36	1	33	0.909
59	48	0.667	48	0.917	43	0.837
60	46	0.870	46	1	46	0.783
61	36	0.861	37	0.946	35	0.943
62	38	0.842	42	1	42	0.905
63	38	0.868	39	0.949	38	0.921
64	31	0.839	32	0.875	32	0.844
65	56	0.911	59	0.983	57	0.965
66	59	0.847	61	0.967	59	0.966
67	29	0.897	28	0.786	27	0.778
68	45	0.933	45	1	44	0.955
69	84	0.893	85	1	76	0.882
70	58	0.914	58	0.966	59	0.983
71	38	0.579	37	0.784	34	0.735
72	50	0.740	52	0.981	52	0.981
73	33	0.818	36	0.944	33	0.970
74	36	0.667	35	0.829	36	0.889
75	40	0.900	42	0.976	43	0.977
76	54	0.889	54	0.981	54	0.963

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